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Space Studies Board

Annual Report

NATIONAL RESEARCH COUNCIL

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1992

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The Space Studies Board is a unit of the National Research Council, which serves as an independent advisor to the federal government on scientific and technical questions of national importance. The Research Council, jointly administered by the National Academy of Sciences, the National Academy of Engineering, and the Institute of Medicine, brings the resources of the entire scientific and technical community to bear through its volunteer advisory committees.

Support for the work of the Space Studies Board and of its committees and task groups was provided by National Aeronautics and Space Administration contract NASW-4627 and National Oceanic and Atmospheric Administration contract 50-DGNE-1-00138.

From the Chair

Since 1958, the Space Studies Board (formerly the Space Science Board) has provided external and independent research and programmatic advice to the U.S. government on the nation's civil space program. This 1992 annual report of the Board records the activities and principal advisory documents issued by the Board during the year.

The year 1992 was an eventful one for the U.S. civil space program. The leadership of the National Aeronautics and Space Administration (NASA) was changed in the first quarter of the year. The new administrator instituted studies of all aspects of the agency's operations and programs, and initiated major management shifts in key program areas, including science, technology, and applications. Once again, there was energetic congressional debate during the summer on the cost and value of such large civil space program elements as the space station, large orbiting observatories, the advanced solid rocket motor, and new launch vehicle concepts. The joint international meeting of the Committee on Space Research (COSPAR) and the International Astronautical Federation (IAF) in Washington in August was the largest such space gathering to date, with a truly outstanding exhibit of space capabilities and achievements from around the globe. Then, elections in November saw a new administration elected by the American public, as well as sweeping changes in the membership of the Congress and of key committees that oversee space and technology.

Other events also had significant implications for the future of space research in a broader scientific and engineering context. Both the National Science Foundation and the National Institutes of Health reassessed their basic missions, raising deep questions about research priorities and national needs. The National Space Council of the outgoing administration issued several policy reports on fundamental aspects of our national space endeavors, military as well as civilian. Rep. George E. Brown, Jr., chair of the House Committee on Science, Space, and Technology, released a report in which he reflected on the state of the U.S. research enterprise and asked penetrating questions about the purposes and future directions of federal support of research. The President's Council of Advisors on Science and Technology (PCAST) and the Federal Coordinating Council for Science, Engineering, and Technology (FCCSET) both issued reports addressing the present and future relationship between the federal government and the research universities. This relationship is visibly stressed, with evidence of declining confidence by the tax-paying public in America's academic institutions.

The year also brought major new accomplishments and results in space research. Some of the highlights included the discovery of large-scale anisotropies in the cosmic background radiation by the Cosmic Background Explorer (COBE) spacecraft, first measurements of Jupiter's magnetosphere by instruments on the joint European Space Agency/NASA Ulysses spacecraft, successful launch and the beginning of data collection by the first Small

Explorer—the Solar, Anomalous, and Magnetospheric Particle Explorer (SAMPEX)—nearly complete radar imaging of the surface of Venus by Magellan, and flight of dedicated U.S. microgravity materials and life sciences laboratories on the space shuttle. The Hubble Space Telescope continued its contributions to astronomy, carrying out studies from our solar system to the most distant reaches of the universe. The intrepid Pioneer and Voyager probes continued their lonely journeys away from the solar system, returning data from further than 50 times the distance from Earth to the Sun. The Mars Observer spacecraft was launched and is on its way to studies of the red planet beginning next year, while Galileo flew by Earth for its last time toward a rendezvous in 1995 with the planet Jupiter.

Underlying all of the programmatic changes and research successes in space is the fundamental fact that a major rethinking—some would call it a major shakeout—is occurring in the space programs of all nations. There is a global reassessment of the place of space activities in individual national priorities, and major restructurings are under way everywhere. Some of the most visible include a decision by the European Space Agency to forgo its independent Hermes piloted program for some years, and large uncertainties about Russia's level of commitment to ambitious plans for robotic exploration of Mars. Russia is also actively marketing many components of its once highly secretive space capability.

There is little doubt that access to space over the past four decades, and the data returned from increasingly sophisticated missions, have provided humankind with a profoundly new vision and understanding of our home on Earth, of the solar system, and of the universe. Space research has been tremendously successful. Although it is seldom acknowledged, there is also little doubt that much space activity around the globe was motivated at the most fundamental level by desires to demonstrate national technological and political supremacy. While not yet clearly articulated or universally accepted, the space research activities of nations in a post-Cold War world will need to be aligned to new national goals that remain ill-defined, but that will certainly be different from those that energized them in the past.

Prioritization of research will be demanded across a wider spectrum of opportunities, programs, and disciplines than in the past. International cooperation—true cooperation—rather than competition and duplication, appears likely to be a central part of a new order. Such true cooperation will not be achieved without creative thinking, good will, and genuine flexibility on all sides. Some implications of the future for U.S. researchers, and their students and colleagues, can only be dimly perceived at present. There will be shifts in space research emphases and major changes in programmatic practices, with contraction possible in many, if not most, space research disciplines. Some of the expected pain can be alleviated through efficiency and ingenuity. But this, too, will require real cultural change and tremendous good will on the part of all.

We researchers, and the Space Studies Board, must be active and innovative participants in helping to define our country's space agenda in a rapidly changing national and global environment—not just for the benefit of science and scientists, but also as a matter of civic duty.

Louis J. Lanzerotti
Chair
Space Studies Board

January 1993

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History and Charter of the Board

ORIGIN OF THE SPACE SCIENCE BOARD

The National Academy of Sciences was chartered by the Congress, under the leadership of President Abraham Lincoln, to provide scientific and technical advice to the government of the United States. Over the years, the advisory program of the institution expanded, leading to the establishment of the National Academy of Engineering and the Institute of Medicine, and of the National Research Council, today's operational arm of the Academies of Sciences and Engineering.

After the launch of Sputnik in 1957, the pace and scope of U.S. space activity grew dramatically. Congress created the National Aeronautics and Space Administration (NASA) to conduct the nation's ambitious space agenda, and the National Research Council (NRC) created the Space Science Board. The original charter of the Board was established in June 1958, three months before final enactment of the legislation creating NASA. The Space Science Board has provided independent scientific and programmatic advice to NASA on a continuous basis from its inception until the present.

REORGANIZATION OF THE BOARD—CREATION OF THE SPACE STUDIES BOARD

In 1988, the Space Science Board undertook a series of retreats to review its structure, scope, and goals. These retreats were motivated by the Board's desire to more closely align its structure and activities with evolving government advisory needs, and by its assumption of a major portion of the responsibilities of the disestablished Space Applications Board. As a result of these retreats, a number of new task groups and committees were formed, and several committees were disbanded and their portfolios distributed to other committees. The Committee on Data Management and Computation and its activities were terminated. The Committee on Planetary Biology and Chemical Evolution was also dismantled, but its responsibilities were distributed to other discipline committees and task groups. The charters of the remaining committees were revised, and an executive council of the Board was created to assist the chair of the Board in managing Board activities.

Recognizing that civilian space research now involves federal agencies other than NASA (for example, the National Oceanic and Atmospheric Administration (NOAA), the Departments of Energy (DOE) and Defense (DOD), and the National Science Foundation (NSF)), it was decided to place greater emphasis on broadening the Board's advisory outreach. This broadening is fully consistent with the Board's founding charter in 1958.

CHARTER OF THE BOARD

The basic elements of the charter of the Board remain those expressed by National Academy of Sciences President Detlev Bronk to Dr. Lloyd Berkner, first chair of the Space Science Board, in a letter of June 26, 1958:

> We have talked of the main task of the Board in three parts—the immediate program, the long-range program, and the international aspects of both. In all three we shall look to the Board to be the focus of the interests and responsibilities of the Academy-Research Council in space science; to establish necessary relationships with civilian science and with governmental science activities, particularly the proposed new space agency, the National Science Foundation, and the Advanced Projects Agency; to represent the Academy-Research Council complex in our international relations in this field on behalf of American science and scientists; to seek ways to stimulate needed research; to promote necessary coordination of scientific effort; and to provide such advice and recommendations to appropriate individuals and agencies with regard to space science as may in the Board's judgment be desirable.
>
> As we have already agreed, the Board is intended to be an advisory, consultative, correlating, evaluating body and not an operating agency in the field of space science. It should avoid responsibility as a Board for the conduct of any programs of space research and for the formulation of budgets relative thereto. Advice to agencies properly responsible for these matters, on the other hand, would be within its purview to provide.

Thus, the Board exists to provide advice to the federal government on space research and to assist in coordination of the nation's space research undertakings. Since its restructuring in 1988 and 1989, the Board has assumed similar responsibilities with respect to space applications. The Board also addresses scientific aspects of the nation's program of human spaceflight.

Recommendations may be prepared either in response to a government request or on the Board's own initiative, and are released after review and approval by the National Research Council. In general, the Board develops and documents its views based on findings of its discipline committees or interdisciplinary task groups that conduct studies of varying duration and extent. These committees and task groups are composed of prominent researchers whose appointments are reviewed and approved by a formal procedure of the NRC. On occasion, the Board itself considers major issues in plenary session and develops its own statements. The Board also provides guidance, based on its publicly established opinions, in testimony to Congress.

The Board's overall scope of activity has several components: discipline oversight, interdisciplinary studies, international activities, and advisory outreach.

OVERSIGHT OF SPACE RESEARCH DISCIPLINES

The Board has responsibility for strategic planning and oversight in the numerous subdisciplines of space research, including space astronomy, earth studies from space, microgravity science, solar and space physics, space biology and medicine, and planetary and lunar exploration. This responsibility is discharged through an organization of separate discipline committees, and includes preparation of strategic research plans and prioritization of objectives as well as assessments of progress in these disciplines. The standard vehicle for providing long-term research guidance is the research strategy report, which has been used successfully by the Board for many years. Committees also prepare formal assessment reports that examine progress in a discipline in comparison with published Board advice. From time to time, in response to a sponsor or Board request, or to circumstances requiring prompt and focused comment, a committee may prepare and submit a brief report in letter format. All committee reports undergo Board and NRC review and approval prior to publication. Board and committee reports are formally issued as reports of the Board and of the National Research Council.

Individual discipline committees may be called upon by the Board, from time to time, to prepare specialized supporting material for use by either the Board or its interdisciplinary committees or task groups.

INTERDISCIPLINARY STUDIES

While the emphasis over the years has been on discipline research planning and evaluation, the reorganization of the Board recognized a need for crosscutting technical and policy studies in several important areas. To address these needs, the Board creates internal committees of the Board and ad hoc task groups. Internal committees, constituted exclusively of Board members, are formed to carry out short-period study activities or to serve as initial planning bodies for topics that may require subsequent formation of a regular committee or task group. Task groups resemble discipline committees in composition and operation, except that they have predetermined lifetimes, typically two to three years, and clearly delimited tasks.

INTERNATIONAL REPRESENTATION

The Board continues to serve as the U.S. National Committee for the International Council of Scientific Unions (ICSU) Committee on Space Research (COSPAR). The U.S. vice president of COSPAR serves as a member of the Board, and a member of the Board's staff serves as Executive Secretary for this office. In this capacity, the Board participates in a broad variety of COSPAR panels and committees.

As the economic and political integration of Europe has progressed, so also has the integration of Europe's space activities. The Board has collaborated successfully with the European space research community on a number of ad hoc joint studies in the past and is now seeking in a measured way to deepen its advisory relationship with this community. To date, the Board's approach has been regular exchange of observers at meetings of the Board and of the European Space Science Committee (ESSC), under the European Science Foundation.

In the future, the Board hopes to initiate cooperative advisory exchanges with the space research programs of Russia and Japan.

ADVISORY OUTREACH

The Space Science Board was conceived to provide space research guidance across the federal government. Over the years, the Board's agenda has focused on NASA's space science program. Since the Board's reorganization, however, several influences have acted to expand the breadth of the Board's purview, both within NASA and outside it.

First, it is recognized that the incorporation in a major way of scientific objectives into manned flight programs such as the shuttle and space station programs, and possibly a Space Exploration Initiative (SEI), necessitates additional interfaces with responsible offices in NASA. The Board is attempting to strengthen its links to the space technology office in NASA through collaborative activities, such as joint workshops, with the NRC's Aeronautics and Space Engineering Board. Stronger links to NASA's space operations, international affairs, and commercial programs offices may also be needed in the future.

Second, the Board's assumption of the space applications responsibilities from the dissolved Space Applications Board has implied a broadening of its advisory audience to other agencies; an example is NOAA, which is responsible for operational weather satellites. In response, NOAA has become a cosponsor of the Board's Committee on Earth Studies.

Third, the maturation of some of the physical sciences may lead to progressive integration of space and nonspace elements, suggesting a more highly integrated advisory structure within the NRC. One example is the solar-terrestrial community, where the Board's Committee on Solar and Space Physics has operated for several years in a "federated" structure with the NRC's ground-based Committee on Solar-Terrestrial Research. Another example is astronomy, where the recently completed report of the NRC's Astronomy and Astrophysics Survey Committee[1] suggests a close relationship between space astronomy and ground-based astronomy, the latter primarily supported by the NSF. The Board therefore established, in 1992, a new Committee on Astronomy and Astrophysics. This committee will operate as a joint committee of the Space Studies Board and the Board on Physics and Astronomy. Other areas of possible future disciplinary association are the general biomedical research community supported by the National Institutes of Health (NIH) and NASA's space biology research program.

Fourth, it has become apparent that new participants may become involved in space exploration, particularly the DOE and the Strategic Defense Initiative Organization (SDIO). Their involvement originates partly from a shared interest in development of space technology, and partly as a result of declassification of some defense technologies in response to the changing geopolitical environment. The SDIO has recently expressed the intention of conducting several space missions of potential scientific interest; the Board has performed an initial assessment of one of these (the Clementine mission to the Moon and an asteroid) and has begun the process of establishing a sponsorship relationship with the SDIO. The Board expects to continue to reach out beyond NASA to other federal agencies, seeking to establish advisory and corresponding sponsorship relationships, where appropriate.

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[1] *The Decade of Discovery in Astronomy and Astrophysics*, Astronomy and Astrophysics Survey Committee, National Academy Press, Washington, D.C., 1991.

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Activities and Membership

During 1992, the Space Studies Board and its twelve committees and task groups gathered for a total of 33 meetings. Three full-length, formal reports were issued, and status assessments were carried out for a number of approved or proposed planetary exploration missions. Letter reports were released on planning for NOAA long-term operational satellites and for DOE facilities used for space radiation research. Since a number of long-term discipline research strategies are becoming obsolete, several committees began work on new strategies. Updated strategies were initiated for solar and space physics and for planetary exploration. After release of its first formal report, a discipline status survey, the Board's new microgravity sciences committee began work on an inaugural research strategy of its own.

The following sections present highlights of the meetings of the Board and its committees during 1992. Formal reports and letter reports developed, approved, and released during these meetings are referenced by the section number of this annual report where their summaries are reprinted (in the case of full-length reports) or they are reproduced in full (in the case of letter reports).

SPACE STUDIES BOARD

The Space Studies Board met four times during 1992, in February, June, August, and November. Between meetings, the Executive Committee of the Board, composed of members of the Board, met several times by teleconference and in person.

In the first meeting of the year, the Executive Committee met on January 13 to act on a number of issues. These included Board appointments; an agenda for the forthcoming February Board meeting; and planning for activities of the Committee on Human Exploration, the Joint Committee on Technology, and the new Committee on International Programs.

The full Board held its 105th meeting, the first during 1992, on February 26-28 in Washington, D.C. Members heard about research results of the previous year's Space Life Sciences (SLS-1) shuttle mission and were briefed by officials of the Space Station Freedom office on station program progress, including current plans for the life sciences centrifuge and associated equipment. A major portion of the meeting was devoted to a series of briefings by NASA's Office of Space Science and Applications (OSSA), NASA Legislative Affairs, and the Congressional Budget Office on the budget outlook for space research. This outlook was far from encouraging. NASA was seeking a 4.7% increase for FY93, but contemporary remarks by Congressman Robert Traxler, chair of the House appropriations subcommittee with jurisdiction over NASA, had indicated that this would be hard to attain. The likelihood was (correctly) foreseen that the $500 million (in FY93 alone) shuttle Advanced Solid Rocket Motor might be reinstated without a corresponding increase in the total NASA budget. In the meantime, it was clear that orderly progress on the Advanced X-ray Astrophysics Facility (AXAF), Cassini, and Earth Observation System (EOS) programs would require significant increases by mid-decade.

At the meeting, the Board approved the final report of its Task Group on Planetary Protection (summarized in Section 3.3). Board members also approved a scientific assessment of the Comet Rendezvous/Asteroid Flyby (CRAF) and Cassini missions prepared by a special subpanel of the Committee on Planetary and Lunar Exploration (COMPLEX), chaired by Prof. Peter Stone of the Massachusetts Institute of Technology. The Board drafted an accompanying letter to the Associate Administrator for Space Science and Applications, Dr. Lennard Fisk, based on the COMPLEX assessment (both reproduced in Section 4.4). Based on the status briefing by Mr. Richard Kohrs, Director of the Space Station Freedom Program, the Board also prepared a statement on this program for the Associate Administrator for Space Systems Development, Mr. Arnold Aldrich (4.3).

An additional letter (4.5) summarizing past Board and committee recommendations on AXAF was later approved by the Executive Committee of the Board and released with these other reports; the whole package was forwarded to NASA Administrator Richard Truly under a general cover letter (4.2).

Many of the Board's committees met during the second quarter of 1992, against the backdrop of a very unsettled space research world. Authorizing subcommittees of both houses of Congress marked up their bills, which were very different in structure and funding. An amendment on the floor of the House to delete the space station was defeated after a passionate debate. Interest in acquiring or sharing capabilities of the space program of the former Soviet Union grew both there and in the United States, and the European space program evolved significantly. The appropriations outlook for the U.S. space program remained poor, as the Congress faced more financing needs than it could fully meet. In the meantime, a new NASA administrator was appointed, Mr. Daniel Goldin, who established agency-wide blue and red team reviews directed at improving the quality and efficiency of programs under way. Restructuring of the Cassini and AXAF missions, begun in 1991 to reduce their funding requirements, continued.

The second 1992 meeting of the Board (its 106th) took place on June 1-3 at the Marshall Space Flight Center (MSFC) in Huntsville, Alabama. Most of the first day was spent on committee business, including review and provisional approval of a number of submitted reports. The Board discussed and approved, pending revisions, a number of letter reports. Two of these, prepared by COMPLEX, assessed the scientific merit of the NASA Office of Exploration robotic lunar precursor missions (4.8) and of SDIO's Clementine Moon/asteroid mission (4.9). A third letter report (4.7), submitted by the Committee on Space Biology and Medicine, discussed the importance of continued operation of the Department of Energy's BEVALAC facility to space radiation biology research. These letter reports were discussed by the Board and received tentative approval, subject to indicated revisions. On the second day of the meeting, NASA officials briefed members on preliminary results of the AXAF restructuring exercise nearing completion, and on the status of the microgravity research program. These briefings were followed by tours of the MSFC AXAF calibration facility and Space Science Laboratory.

The Space Studies Board met for its 107th meeting on August 28-29 at the National Academy of Sciences in Washington, D.C. This one-and-a-half-day meeting was chiefly devoted to committee business, including discussion of plans for initiation of the new Committee on Astronomy and Astrophysics, to be operated jointly with the NRC's Board on Physics and Astronomy. The Board reviewed its international program, with several members planning to attend the September 1992 meeting of the European Space Science Committee (ESSC). During lunch on the first day of the meeting, the Board heard luncheon remarks by NASA Administrator Goldin on the red and blue team reviews in progress and on his goals for the agency. This talk was followed by a briefing by Dr. Fisk of OSSA on the status of the space science and applications program. The Board later reviewed a second COMPLEX letter report (4.10) on the Cassini program, this time considering the capabilities of the restructured mission (CRAF canceled and Cassini redesigned), and returned the report to the committee with provisional approval and directions for revision and augmentation.

Several significant events occurred between the August and late November Board meetings: the conference appropriation bill for NASA and related agencies was passed by the Congress and signed by the President on October 5, and Administrator Goldin announced sweeping changes in NASA's organization on October 15.

The final appropriation for NASA conformed largely to expectations from the debate and legislative activity earlier during the year. In the end, NASA received a total of $14,330 million, which was $663 million less than the President's FY93 request, but only $4 million less than the FY92 appropriation. OSSA, on the other hand, experienced funding growth of $127 million over the FY92 level (about a 4.7% increase). While less than hoped for in the President's request, OSSA's appropriated amount was expected to enable forward progress on major research missions such as Cassini, AXAF, and EOS, with some reductions in scope and capability to each of these programs. The CRAF mission remained terminated, as provided in the President's budget request.

While the implications of the FY93 appropriation could be fairly well understood in October, the consequences of Administrator Goldin's reorganization were less apparent. The major provision of the reorganization affecting space research was the fragmentation of OSSA and the reassignment of Dr. Fisk, its director, to the position of NASA Chief Scientist. The Earth Science and Applications Division was elevated to become the Office of Mission to Planet Earth; the Solar System Exploration Division, Astrophysics Division, and Space Physics Division were gathered into a new Office of Planetary Science and Astrophysics; and the Life Sciences Division and the Microgravity Science and Applications Division remained unassigned for the moment. In view of its charter to provide long-term strategic research advice to the whole of NASA's science and applications portfolio, the Board had a keen interest in both the administrator's reasons for these sweeping changes and the effects they were likely to have on initiation and execution of space research missions.

In response to these developments, the Executive Committee of the Board held a teleconference on November 5 to set the agenda for the Board meeting planned for mid-November. The Executive Committee resolved to invite Mr. Goldin to present the reorganization and his views at the meeting, and also to solicit Dr. Fisk's perspectives as well. The committee met again informally on November 17 to review what had become known about the reorganization and to assemble specific issues for discussion with the administrator at the Board meeting the following day.

The Space Studies Board held its last meeting of 1992, its 108th, at the Beckman Center in Irvine, California, on November 18-20. The agenda for the meeting included committee reports and approvals, consideration of broad policy issues, and discussion of Board plans for 1993. Highlights of the meeting were briefings by Administrator Goldin (in person) and a teleconference with Dr. Fisk. Mr. Goldin gave a broad perspective on his motivations for the reorganization and fielded questions from the members on a variety of space research-related concerns. Dr. Fisk added clarifications in a number of areas the following day via teleconference.

In other activities at the meeting, the Board was briefed by Committee on Solar-Terrestrial Research Chair Donald Williams about the results of the joint study by that committee and the Board's Committee on Solar and Space Physics on trends in research support. In subsequent discussion, the Board approved both a new charge for the Committee on Earth Studies and the Phase 2 report of the Committee on Human Exploration, which describes science objectives that could be enabled or materially enhanced by a human exploration program. The Board was privileged to hear from three members of the ESSC, Drs. Heinrich Völk, Herb Schnopper, and François Becker, who discussed their committee's status and plans, with emphasis on their recent report on satellite Earth observation. The Board viewed several videos on NASA spinoffs into medical technology, heard an analysis of last summer's workshop by the Task Group on Priorities in Space Research, and approved the Board's *1993 Operating Plan*.

Membership of the Space Studies Board

Louis J. Lanzerotti,§ AT&T Bell Laboratories (chair)
Joseph A. Burns, Cornell University
Andrea K. Dupree,* Harvard-Smithsonian Center for Astrophysics
John A. Dutton, Pennsylvania State University
Anthony W. England, University of Michigan
Larry W. Esposito,* University of Colorado
James P. Ferris, Rensselaer Polytechnic Institute
Herbert Friedman, Naval Research Laboratory
Richard L. Garwin,* IBM T.J. Watson Research Center
Riccardo Giacconi, European Southern Observatory
Noel W. Hinners,§ Martin Marietta Civil Space and Communications Company
James R. Houck,* Cornell University
David A. Landgrebe, Purdue University
Robert A. Laudise, AT&T Bell Laboratories
Richard S. Lindzen,§ Massachusetts Institute of Technology
John H. McElroy, University of Texas at Arlington
William J. Merrell, Jr., Texas A&M University
Richard K. Moore,* University of Kansas
Robert H. Moser, University of New Mexico

Norman F. Ness,§ University of Delaware
Marcia Neugebauer, Jet Propulsion Laboratory
Simon Ostrach, Case Western Reserve University
Carle M. Pieters,§ Brown University
Mark Settle, ARCO Oil and Gas Company
William A. Sirignano, University of California at Irvine
John W. Townsend, Jr., NASA (retired)
Fred Turek, Northwestern University
Arthur B.C. Walker, Stanford University

Duane T. McRuer, Systems Technology, Inc. (ex officio)
Donald J. Williams, Johns Hopkins University (ex officio)

Marc S. Allen, Director
Betty C. Guyot, Administrative Officer

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*term expired during 1992
§member of the Executive Committee

COMMITTEE ON INTERNATIONAL PROGRAMS

Committee on International Programs (CIP) Chair William Merrell led a delegation consisting of Committee on Earth Studies Chair John McElroy and Board Director Marc Allen to the European Space Science Committee (ESSC) meeting in Budapest on September 23-24. Among other actions, the ESSC heard a briefing on the European Space Agency (ESA) science program and approved a draft report by its subpanel, the European Earth Observation Panel. This report was subsequently published as *A Strategy for Earth Observation from Space* (European Science Foundation, September 1992). Board attendees at the Budapest meeting discussed ways to better coordinate advisory work between the U.S. and European committees.

Through the efforts of Board member and U.S. COSPAR Vice President Herbert Friedman, and staff member and U.S. COSPAR Executive Secretary Richard Hart, the Board continued its active participation in international COSPAR activities. The COSPAR Bureau met on August 27 and September 4; the COSPAR Executive Council met on August 28 and September 5. Most of the activities during the year were dedicated to organizing the 1992 plenary meeting that was held in Washington, D.C., from August 28 to September 5 in conjunction with the meeting of the International Astronautical Federation. This joint meeting was known as the World Space Congress—a major event of the International Space Year. The congress was attended by over 5000 participants, representing 65 countries. In addition, 135 organizations participated in the exhibit—the largest ever for a COSPAR meeting.

At the congress, a new COSPAR charter and bylaws were approved by COSPAR. If approved by the International Council of Scientific Unions (ICSU), they will go into effect in 1994. The most important change is that, beginning in 1994, all officers will be elected from a slate prepared by a nominating committee.

While COSPAR appears to be in a sound state at present, there are uncertainties for the near future given recent changes in the world. It is not clear that all of the member nations will be able to continue to pay their national contributions. On the other hand, there may be several new members of COSPAR in the near future: both the People's Republic of China and China Taipei seem to be seriously interested; Korea has recently launched a satellite; and a number of the new independent republics of the former Soviet Union are considering joining.

CIP Membership

William J. Merrell, Jr., Texas A&M University (chair)
Herbert Friedman, Naval Research Laboratory
James R. Houck,* Cornell University

Richard C. Hart, Executive Secretary

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*term expired during 1992

JOINT COMMITTEE ON TECHNOLOGY FOR SPACE SCIENCE AND APPLICATIONS

The Joint Committee on Technology (JCT) for Space Science and Applications, a collaborative effort between the Board and the NRC's Aeronautics and Space Engineering Board, held a week-long workshop in June in Annapolis, Maryland. The workshop, which was chaired by Dr. John McElroy, reviewed NASA plans for developing new space technologies in support of future science and applications programs. Twenty-six scientists and engineers met to receive briefings from representatives of NASA's Office of Aeronautics and Space Technology (OAST) and OSSA. A report summarizing the findings of the workshop was prepared and is expected to be released in early 1993. The committee also considered candidate topics for future workshops.

JCT Membership

David A. Landgrebe, Purdue University (co-chair)
John M. Hedgepeth,§ consultant, Santa Barbara (co-chair)
John H. McElroy,* University of Texas at Arlington (workshop chair)
Andrea K. Dupree,* Harvard-Smithsonian Center for Astrophysics
Duane T. McRuer,§ Systems Technology, Inc.
Franklin K. Moore,§ Cornell University
Richard K. Moore,* University of Kansas

Richard C. Hart, Executive Secretary
Noel E. Eldridge, Executive Secretary

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*term expired during 1992
§member, Aeronautics and Space Engineering Board

COMMITTEE ON ASTRONOMY AND ASTROPHYSICS and TASK GROUP ON AXAF

After a hiatus of over four years, astronomy and astrophysics returned to committee status within the Space Studies Board. The NRC determined that a unified space and ground astronomy committee is the best approach to continuing advisory oversight of these disciplines. Establishment of the Committee on Astronomy and Astrophysics (CAA), a new joint activity of the Board and the NRC's Board on Physics and Astronomy (BPA), was formally approved by the NRC Governing Board in May 1992. The CAA will cover ground- and space-based astronomy and astrophysics and will be the NRC's advisory body for these disciplines. A letter signed by Board Chair Louis Lanzerotti and BPA Chair Frank Drake was sent out to solicit suggestions for CAA nominees, and returned over 100 nominations. Prof. Marc Davis agreed to chair the committee. Other appointments were finalized, and the committee will meet in January 1993. Additional support and sponsorship for the joint committee are being sought from the National Science Foundation.

Simultaneously, the Space Studies Board initiated a Task Group on AXAF (TGA), chaired by Prof. Arthur Davidsen. Anticipating a need for rapid NRC review of the science responsiveness of the restructured AXAF, task group members were appointed and a meeting planned to prepare a letter report. The TGA solicited and received comments from the x-ray astronomy community and held several planning teleconferences. The committee subsequently met on December 10-11, 1992, to review and evaluate the restructured AXAF mission. Participants included NASA managers and head scientists, as well as a number of principal investigators for the mission. The TGA considered whether the restructuring met the scientific rationale as envisioned in previous reports from the Space Studies Board and from NRC astronomy survey committees. The final TGA-approved draft is expected to be ready for Board and NRC review in February 1993. The target delivery date for the letter report is early 1993.

CAA Membership

Marc Davis, University of California at Berkeley (chair)
Arthur F. Davidsen, Johns Hopkins University
Sandra M. Faber, Lick Observatory
Holland C. Ford, Space Telescope Science Institute
Jonathan E. Grindlay, Harvard University

Doyal A. Harper, Jr., Yerkes Observatory
Kenneth I. Kellermann, National Radio Astronomy Observatory
Richard A. McCray, Joint Institute Laboratory for Astrophysics
Jeremiah P. Ostriker, Princeton University Observatory
Bernard Sadoulet, University of California at Berkeley

Robert L. Riemer, Executive Secretary

TGA Membership

Arthur F. Davidsen, Johns Hopkins University (chair)
David W. Arnett, University of Arizona
Hale Bradt, Massachusetts Institute of Technology
Anne P. Cowley, Arizona State University
Paul Gorenstein, Smithsonian Institution
Steven M. Kahn, University of California at Berkeley
James D. Kurfess, Naval Research Laboratory

Robert L. Riemer, Executive Secretary

COMMITTEE ON EARTH STUDIES

The Committee on Earth Studies (CES) met on February 10-12 to consider NOAA's requirements for future polar-orbiting operational environmental satellites. Based on briefings by NOAA managers and committee deliberations, CES prepared a letter report (4.6) containing recommendations for strengthening this planning document. The committee was also updated on NASA earth studies programs, including EOS.

On June 17-19, CES met in Washington, primarily to consider the role of satellite measurements in numerical modeling. A number of briefers from research and operational centers described the use of these data and the impact of improvements in collected data on model outputs. Members heard about the status of NOAA programs from Mr. Russell Koffler and about NASA Earth observing programs from Dr. Shelby Tilford, and witnessed a demonstration of microcomputer-based ground station technology. Members of the committee also discussed the Landsat program.

The committee's final meeting of the year was held November 16-17 at the Beckman Center. In preparation for defining its next major task, briefings were heard about a wide range of Earth remote sensing proposals and programs of the U.S. Air Force, Sandia National Laboratories, DOE, NOAA, and NASA. Recent management changes at NASA Headquarters and their effects on the earth science and applications program were discussed. The committee heard a presentation on the purpose, status, and goals of the congressionally mandated Consortium for International Earth Science Information Network (CIESIN) and discussed various options for its next major task. The resulting proposal, which was submitted to the Board and approved at its November meeting, provides for the committee to update its report, *Assessment of Satellite Earth Observation Programs—1991*, and to conduct a study identifying critical issues in U.S. earth sciences and applications programs.

CES Membership

John H. McElroy, University of Texas at Arlington (chair)
George Born, University of Colorado
Janet Campbell, Bigelow Laboratories
Dudley Chelton, Jr., Oregon State University
Charles Elachi,* Jet Propulsion Laboratory
William J. Emery,* University of Colorado
Diana Freckman, University of California at Riverside
Richard E. Hallgren,* American Meteorological Society
Kenneth Jezek, Ohio State University
Edward Kanemasu, University of Georgia
Vytautas (Victor) Klemas,* University of Delaware
Richard Kott, consultant, Fort Washington, Maryland

Conway Leovy, University of Washington
John MacDonald, MacDonald-Dettwiler Associates
Alfredo E. Prelat,* Texaco Corporation
Clark Wilson, University of Texas at Austin

Joyce M. Purcell, Executive Secretary
David H. Smith, Executive Secretary

*term expired during 1992

COMMITTEE ON HUMAN EXPLORATION

The Committee on Human Exploration (CHEX) met on January 21-22 in Denver to discuss plans for a study on alternative approaches to science management within human spaceflight programs. At this meeting, the committee was briefed on current planning by representatives from NASA's Space Station Freedom and Exploration offices. The committee met again for a summer workshop at the Academy's Woods Hole Center on July 6-10. Members continued work on the science management report, making good progress.

CHEX met at the Academy's Foundry facility in Washington on October 22-23 to complete its second report, *Scientific Opportunities in the Human Exploration of Space*, and to continue work on the science management report. The major action taken to finish the science opportunities report was to ensure that the scientific activities singled out by the Board's collaborating discipline committees as those especially enabled by a human exploration program were consistent with a series of general guidelines for scientific participation in human exploration developed in the committee's first, and completed, report on the science that must be done to enable a program of extended human spaceflight. A draft of the *Opportunities* report was submitted to the Board for approval at its November meeting in Irvine, on November 18-20, and was approved pending minor changes. The report was revised by the committee and forwarded to the Board's Executive Committee, which gave final approval in December.

The committee's first report, *Scientific Prerequisites for the Human Exploration of Space*, is expected to go to the printer in February. The science management report should be completed early in 1993.

CHEX Membership

Noel W. Hinners, Martin Marietta Civil Space and Communications Company (chair)
Richard L. Garwin,* IBM Corporation
Louis J. Lanzerotti, AT&T Bell Laboratories
Elliott C. Levinthal, Stanford University
William J. Merrell, Jr., Texas A&M University
Robert H. Moser, University of New Mexico
John E. Naugle, consultant, North Falmouth, Massachusetts
George D. Nelson,* University of Washington
Marcia S. Smith, Congressional Research Service
Gerald J. Wasserburg, California Institute of Technology

David H. Smith, Executive Secretary

*term expired during 1992

COMMITTEE ON MICROGRAVITY RESEARCH

The Committee on Microgravity Research (CMGR) met February 24-25 to continue work on its new strategy for microgravity science. The strategy will be a long-range, comprehensive science road map to help guide NASA's future research in the field. The CMGR intends to make an initial presentation of the strategy report to the Board in February 1993. The committee also reviewed microgravity applications activities of NASA's Office of Commercial Programs.

At a subsequent meeting at the Lewis Research Center in Cleveland, Ohio, on May 20-21, members reviewed the microgravity activities and facilities at the center and continued work on their strategy for microgravity research.

The committee met again on July 21-22, September 21-22, and November 16-17, 1992, to work on the strategy report. In addition, at the November meeting, the committee considered NASA's plans for the space station centrifuge facility.

CMGR Membership

William A. Sirignano, University of California at Irvine (chair)
Robert A. Brown,* Massachusetts Institute of Technology
Howard M. Einspahr, The Upjohn Company
Martin E. Glicksman, Rensselaer Polytechnic Institute
Franklin D. Lemkey, United Technologies Research Center
Ronald E. Loehman, Sandia National Laboratories
Alexander McPherson, University of California at Riverside
Simon Ostrach, Case Western Reserve University
Morton B. Panish, AT&T Bell Laboratories
John D. Reppy, Cornell University
Thomas A. Steitz,* Yale University
Warren C. Strahle, Georgia Institute of Technology
Julia R. Weertman, Northwestern University

Richard C. Hart, Executive Secretary

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*term expired during 1992

COMMITTEE ON PLANETARY AND LUNAR EXPLORATION

The Committee on Planetary and Lunar Exploration (COMPLEX) held a major meeting on February 18-21 at the Beckman Center. The first two days were devoted to briefings on, and a scientific review of, the baseline (pre-restructure, but after deletion of the penetrator) CRAF and Cassini missions. The results of this review were documented in an assessment letter report (4.4) and later released by the Board. The second part of the meeting was devoted to planning for an integrated solar system research strategy to be developed by the committee over the next two years.

The committee met again in Washington on April 27-28. Members were briefed by SDIO and NASA officials on the joint Clementine Moon/asteroid mission. Members of Clementine's science working group described progress in selecting the filters for the mission instrumentation, enabling COMPLEX members to assess the potential science return in a letter report (4.9). NASA Associate Administrator for Exploration Michael Griffin described payload characteristics of the Office of Exploration's proposed lunar precursor missions, which were also compared to previous COMPLEX measurement objectives. These assessments (4.8) were submitted for Board and NRC approval and released.

COMPLEX members gathered again at the Beckman Center on July 13-17 to begin work on their new project—developing a unified set of scientific priorities for the planetary sciences over the next 10-15 years. This new strategy will replace COMPLEX's existing separate strategies for the inner planets, outer planets, primitive bodies, and search for other solar systems. Rather than dividing the solar system into general regions and examining them separately, this new study will take a comprehensive look at the solar system and divide it into scientific disciplines such as planetary atmospheres, surfaces and interiors, magnetospheres, rings, dust and primitive bodies, and the formation of the solar system and the origins of life. During this July meeting, COMPLEX members also conducted a reassessment of the reconfigured Cassini mission, including the effects of the termination of CRAF, and submitted a draft report to the Space Studies Board at its August meeting.

At a fourth meeting on September 21-23 at NASA's Goddard Space Flight Center, COMPLEX continued work on its integrated research strategy. COMPLEX received briefings on planetary magnetospheres that were omitted from the July meeting agenda because of schedule conflicts. The committee was also briefed on the status of the parallel priority-setting exercise being undertaken by the CSSP/CSTR. In addition to working on its integrated strategy, the committee also tackled a number of other tasks at the September meeting. Other topics included future missions to Mercury, the use of the Hubble Space Telescope (HST) for planetary observations, and the results of a recent study on the mitigation of asteroid hazards. COMPLEX members toured Goddard instrument laboratories, viewed hardware being constructed for the HST repair mission, and visited the control room of the International Ultraviolet Explorer. A portion of the meeting was devoted to minor revisions to the letter report drafted in July on the restructuring of the Cassini mission; the letter (4.10) was subsequently approved and released to NASA on October 19.

As part of the outreach activities for its priority-setting study, members of COMPLEX gave progress reports at a number of international scientific conferences. Prime among these was the October 13-17 meeting of the American Astronomical Society's Division for Planetary Sciences in Munich, Germany. In an evening session on October 15th, COMPLEX Chair Joseph Burns briefed the conference participants on the charge given to COMPLEX by the Board. He outlined events at the summer workshop in Irvine and progress the committee had made so far. Participation of a representative of the ESSC at the July meeting and the desire for additional European participation in the priority-setting study were emphasized. Following a description of the committee's plans for future meetings, Prof. Burns took questions from the audience during the limited additional time available.

COMPLEX made a second progress report (similar in format to the one given in Munich) at a meeting of the American Geophysical Union in San Francisco in December. The presentation was made by COMPLEX member Prof. William Kurth. The next major outreach activity is scheduled to take place at the Lunar and Planetary Science Conference in Houston, Texas, in March 1993.

COMPLEX Membership

Joseph A. Burns, Cornell University (chair)
Reta F. Beebe, New Mexico State University
Alan P. Boss, Carnegie Institution of Washington
Geoffrey A. Briggs, NASA Ames Research Center
Michael H. Carr, U.S. Geological Survey
Anita L. Cochran, University of Texas at Austin
Thomas M. Donahue, University of Michigan
James L. Elliot, Massachusetts Institute of Technology
Larry W. Esposito,* University of Colorado (former chair)
Peter J. Gierasch, Cornell University
John F. Kerridge, University of California at Los Angeles
William S. Kurth, University of Iowa
Barry H. Mauk, Applied Physics Laboratory
Lucy-Ann A. McFadden,* University of California at San Diego
Christopher P. McKay,* NASA Ames Research Center
William B. McKinnon, Washington University
Duane O. Muhleman,* California Institute of Technology
Norman R. Pace, Indiana University
Graham Ryder, Lunar and Planetary Institute
Paul D. Spudis,* Lunar and Planetary Institute
Peter H. Stone,* Massachusetts Institute of Technology
Darrell F. Strobel, Johns Hopkins University
George W. Wetherill, Carnegie Institution of Washington
Richard W. Zurek,* California Institute of Technology

David H. Smith, Executive Secretary

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*term expired during 1992

COMMITTEE ON SPACE BIOLOGY AND MEDICINE

The Committee on Space Biology and Medicine (CSBM) met on February 13-14 and heard presentations on results from the first Space Life Sciences (SLS-1) mission, the first International Microgravity Laboratory (IML-1) mission, and plans for SLS-2. Committee members discussed current life sciences research issues with congressional staff and NIH officials.

The committee met again on May 14-15 in Washington. In an effort to broaden its understanding of the capabilities and research potential of various former Soviet Union space facilities, such as the space station Mir and the Cosmos biosatellite series, the committee devoted a significant amount of the meeting to discussing these topics. Other items discussed were cooperative activities between NASA and NIH and long-term planning for NASA's radiation research program. The committee drafted a letter report (4.7) on the importance of continued operation of the BEVALAC facility for space radiation biology. The letter was later approved by the Board and the NRC and released.

The CSBM met for a third time on September 30 and October 1, at the Beckman Center, and discussed a number of fundamental issues pertinent to the Life Sciences Division's research program, particularly peer review and the status and plans for the division's Discipline Working Groups (DWGs). More results derived from the IML-1 mission were presented and plans for SLS-2 were discussed. A substantial portion of the meeting was devoted to further discussion about use of Russian space facilities such as Mir and the Cosmos biosatellites for U.S. life sciences research. The committee also viewed and evaluated two videos describing NASA life sciences research facilities and claimed terrestrial benefits.

CSBM Membership

Fred W. Turek, Northwestern University (chair)
Robert M. Berne,* University of Virginia at Charlottesville
Robert E. Cleland, University of Washington
Mary F. Dallman, University of California
John R. David, Harvard School of Public Health
Peter Dews,* Harvard Medical School
R.J. Michael Fry,* Oak Ridge National Laboratory
Francis (Drew) Gaffney, University of Texas, Southwestern Medical Center
Edward J. Goetzl,* University of California Medical School at San Francisco
Marc D. Grynpas, Mount Sinai Hospital, Toronto
Robert Helmreich, University of Texas at Austin
James Lackner, Brandeis University
Robert W. Mann, Massachusetts Institute of Technology
Clinton T. Rubin, State University of New York at Stony Brook
Fred D. Sack, Ohio State University
Alan L. Schiller,* Mt. Sinai Medical Center
Tom Scott,* University of North Carolina at Chapel Hill
Warren Sinclair, National Council on Radiation Protection and Measurements
William Thompson,* North Carolina State University
Fred Wilt, University of California at Berkeley

Joyce M. Purcell, Executive Secretary

*term expired during 1992

COMMITTEE ON SOLAR AND SPACE PHYSICS

The Committee on Solar and Space Physics (CSSP), which operates in a "federated" structure with the Committee on Solar-Terrestrial Research (CSTR) of the NRC's Board on Atmospheric Sciences and Climate, met on March 16-18 at the Beckman Center to continue its study on the balance between "big" and "little" research programs within the committees' disciplines. The resulting *Report on a Space Physics Paradox* will address the question: Why has increased funding been accompanied by increased dissatisfaction in the research community? The federated committee planned for its new research strategy project and reviewed the status of its study on atmospheric electricity. At this meeting, the committees also prepared inputs to the CHEX science opportunities study and to the summer workshop of the Task Group on Priorities in Space Research.

The federated committees met for a week in July (26-31) to finish writing the *Paradox* report and to begin work on the new strategy. About a dozen outside scientists were invited to attend as guests to help formulate an approach to the strategy.

At a third meeting, held on October 19-21, the committees approved the *Paradox* report, continued work on the strategy study, reviewed relevant agency activities (NASA, NSF, NOAA, DOD), reviewed a draft report by the Panel on Solar Influences of the NRC's Committee on Global Change Research, reviewed the activities of the U.S. Coordination Office for the Solar-Terrestrial Energy Program (STEP), and discussed the recent reorganizations of the National Space Science Data Center and of NASA. The *Paradox* report was presented to the Board at its November meeting.

Subsequently, CSSP/CSTR organized a special session during the December meeting of the American Geophysical Union (AGU) in San Francisco to solicit suggestions for the new research strategy. Approximately 250-300 members of the AGU Space Physics and Aeronomy Section attended the session, and a number of contributions were received.

CSSP Membership

Marcia Neugebauer, Jet Propulsion Laboratory (chair)
Thomas E. Cravens, University of Kansas
Jonathan F. Ormes, Goddard Space Flight Center
George K. Parks, University of Washington
Douglas M. Rabin, National Optical Astronomy Observatory
David M. Rust, Johns Hopkins University
Raymond J. Walker, University of California at Los Angeles
Yuk L. Yung, California Institute of Technology
Ronald D. Zwickl, National Oceanic and Atmospheric Administration

Richard C. Hart, Executive Secretary

TASK GROUP ON PLANETARY PROTECTION

The Task Group on Planetary Protection (TGPP) published its report, *Biological Contamination of Mars: Issues and Recommendations*, in time for use at the World Space Congress' meeting of COSPAR held in Washington in late August. COSPAR has the responsibility for monitoring and updating planetary protection policy for all space-faring nations. Upon publication of its report, the task group was disbanded.

*TGPP Membership**

Kenneth H. Nealson, University of Wisconsin-Milwaukee (chair)
John Baross, University of Washington
Michael Carr, U.S. Geological Survey
Robert Pepin, University of Minnesota
Thomas Schmidt, Miami University
Jodi Shann, University of Cincinnati
J. Robie Vestal, University of Cincinnati
David White, Oak Ridge National Laboratory
Richard Young, consultant, Kennedy Space Center

Joyce M. Purcell, Executive Secretary

*task group disbanded during 1992

TASK GROUP ON PRIORITIES IN SPACE RESEARCH

Early in 1992, the Board published the first report from its Task Group on Priorities in Space Research (TGPSR), chaired by Board member Dr. John Dutton. This report, entitled *Setting Priorities for Space Research: Opportunities* *and Imperatives*, was released at a half-day symposium held in the National Academy of Sciences (NAS) Auditorium on January 24. NAS President Frank Press opened the symposium and commented on the desirability of addressing priority issues in science: "One has to do this not simply because there are budgetary constraints, but also as a means of self-examination . . . it gives the public confidence that we're going about our job right if we say to the public, who, after all pays for all of this, that we have examined and ranked our needs and opportunities." Mr. George E. Brown, Jr., chair of the House Committee on Science, Space, and Technology, gave the keynote address. Applauding the endeavors of the task group and the Board, Mr. Brown encouraged them and the scientific community at large to provide policymakers with a best assessment of priority ordering based on "unadulterated peer-reviewed judgment of scientific merit." Board Chair Louis Lanzerotti discussed how and why the Board and the space research community should be involved in recommending priorities. These remarks were followed by an open forum in which members of the audience were asked for their views and reactions to plans for a follow-on study—development of a priority-setting methodology and evaluation criteria.

In February, the Board sponsored a session on the topic of priority setting in research at the American Association for the Advancement of Science meeting in Chicago.

Congressional interest in the topic continued, with Dr. Dutton being asked to testify in April on priorities in space research before the House Science, Space, and Technology Committee's Subcommittee on Science.

During a July 27-30 workshop at Woods Hole, the task group tested and validated its model space research proposal and evaluation instrument. Outside guests, representing some of the major disciplines of space research, evaluated representative projects using the prototype proposal and evaluation instruments to gauge how effective and valid their application to actual projects would be. The task group began work on a first draft of a follow-on report, which will describe possible methods for setting priorities in space research fields and will discuss the committee's prototype instruments.

The task group held a meeting on October 22-23 to discuss further the results and implications of the Woods Hole experiment. Both the proposal questionnaire and evaluation method were modified as a result of the experiment. The group continued working on its follow-on report with the aim of completing the activity by June 1993.

TGPSR Membership

John A. Dutton, Pennsylvania State University (chair)
Philip Abelson, American Association for the Advancement of Science
William P. Bishop, Desert Research Institute
Lawson Crowe, University of Colorado
Peter Dews, Harvard Medical School
Angelo Guastaferro, Lockheed Missiles and Space Company, Inc.
Molly K. Macauley, Resources for the Future
Buddy MacKay,* Lt. Governor of Florida
Thomas A. Potemra, Johns Hopkins University
Arthur B.C. Walker, Stanford University

Joyce M. Purcell, Executive Secretary

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*term expired during 1992

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Summaries of Reports

3.1 Setting Priorities for Space Research: Opportunities and Imperatives

A Report of the Task Group on Priorities in Space Research[1]

[Policy] is like a play in many acts, which unfolds inevitably once the curtain is raised. To declare that the performance will not take place is an absurdity. The play will go on, either by means of the actors . . . or by means of the spectators who mount the stage.

Klemens von Metternich, 1880

The U.S. space program and its space research components have produced remarkable achievements in the past three decades and generated a wealth of opportunities for scientific initiatives in the years ahead. As we approach a new century, we must decide: What should we do? How should we do it?

Answers to these questions are critical for the future success of the space program and space research (that is, scientific activities concerned with phenomena in space or utilizing observations made in, or from, space). The answers will affect the strength of the national scientific and engineering enterprise, national economic vitality, and the national sense of pride and purpose. Answering the first question is equivalent to setting priorities for space research. Answering the second question requires that we develop a model for our activities that will facilitate accomplishing our highest-priority activities. Priorities, as used here, are rankings in a preferential ordering or agenda, possibly multidimensional, that governs allocation of resources to activities or initiatives.

For some time, the objectives of the space research community and those of the broader space program have been in conflict. Apollo demonstrated national technological superiority at a critical time. A fundamental assumption of the civil space program developed in that era asserts that it is human destiny to explore the universe. As a consequence, the civil space program continues to emphasize the mechanical aspects of flying spacecraft and transporting humans through space. In contrast, scientific vision focuses on the outcome of space activities, insisting that the means of conducting scientific research be determined by the objectives and purposes of that research itself; it emphasizes the information and understanding generated rather than the means of obtaining them.

New realities of international competition, domestic politics, and economics suggest the need to review the contributions of space research to national vitality. The accomplishments of the past and the many opportunities now

[1] "Summary and Recommendations" reprinted from *Setting Priorities for Space Research: Opportunities and Imperatives*, National Academy Press, Washington, D.C., 1992, pp. 1-8.

available, as well as the widely recognized need to provide stimulation and motivation to education, suggest that we reconsider how scientific research in space is conducted. Fundamental assumptions about the objectives of space research and the space program that makes it possible may determine the outcome of research more than judgments about scientific merit, or national values, or imperatives presented by the new realities mentioned above. Thus the issue is not the relative value of the human spaceflight and space research components of the space program. Rather, it is to develop objectives and operating principles that will produce the maximum benefits from the nation's investment in space research and other space activities.

The imperative driving scientific research is the acquisition of knowledge and understanding. The collection of data, the creation of information through its analysis, and the subsequent development of insight and understanding should be key governing objectives for scientific research in space and for the broader objective of the space program. As suggested in the preface, the task group believes that this vision is compatible with the human spaceflight program and that the entire space program itself would be invigorated by concentrating on timely and compelling scientific objectives.

Emphasizing information and understanding will not compromise the overall space program's legitimate interest in the technology of spaceflight because formidable engineering and technical challenges must be met in order for space research to achieve its objectives. It will, however, permit the space research program and the overall space program to concentrate on the development of powerful new techniques for acquiring, communicating, synthesizing, and using information. And because information itself is an increasingly critical and economically valuable resource, this effort can enhance our national technological progress and economic strength while it enhances our scientific accomplishments.

Thus the vision of a space program and a space research effort emphasizing information, knowledge, and understanding presents an ideal format in which to consider priorities for space research. The central thesis of this report is that the space science and applications community should reach a consensus on priorities for scientific research in space. Since we cannot do everything, we should do the most valuable things, with the recognition that a collection of smaller efforts may in sum be more important than a single large initiative. The task group believes that a scientific agenda set forth by the community, with due regard for contemporary political and economic realities, will greatly assist policy makers and will ultimately prevail. Such an agenda, along with the reformulation of assumptions governing space research, will better serve scientific and national goals, achieve maximum return on investment, encourage effective congressional and agency action, and provide benefits for the nation's citizens.

ACCOMPLISHMENTS, PROSPECTS, AND LESSONS FROM THE U.S. SPACE RESEARCH PROGRAM

The accomplishments since 1957 of U.S. scientific research in space have broadened and deepened understanding of our physical environment. As with all science, these accomplishments are but harbingers of even greater future achievements. Past successes have created a multiplicity of opportunities for space science and applications. Moreover, our more than 30 years of experience in space research has provided important lessons on how to operate the program more effectively in order to obtain the maximum possible benefit from available resources.

All disciplines reveal the complexity of the physical and biological world. Things are much more complicated than we thought at the beginning of the space age in 1957. As examples, consider the violent astronomical events, the courses of planetary evolution, the interactions of solar and terrestrial magnetic processes, the interdependence of the various components of the Earth system, and the changes in human physiology that occur in space. We can expect to discover even more variety and more complexity in the years ahead.

Perhaps the most striking accomplishment of the U.S. space program is the demonstration that humans can work in space and on another body of the solar system and can travel to another part of the solar system and return successfully. This demonstration has opened the way for human exploration beyond the Earth for centuries to come.

The value of the unique point of view attainable from space has been demonstrated beyond doubt. We gain more than just a different perspective: operating far from the Earth's surface expands the domain of parameters available to science. This expansion will continue with the return and analysis of samples from planets, asteroids, and comets, with observations that reach back even further toward the origins of the universe, with extended human presence in space, and with comprehensive views of the interactions of the Earth's physical and biological subsystems.

In over 30 years of experience in space research, we have learned that flexibility and multiplicity of opportunity are key requirements. Although large missions may address the most urgent or most comprehensive scientific issues, small or moderate missions and suborbital initiatives can also resolve important scientific questions, and can do so more quickly and less expensively. For space research to produce maximum benefits, the objectives of scientific research should drive the mission rather than constraints imposed by the limitations of a program or a particular launch vehicle.

TODAY'S IMPERATIVES

Recent events at home and abroad require that we reexamine motivations, objectives, and methods of space research to ensure that they are responsive to contemporary imperatives. The key imperatives and their implications are as follows:

- Rapidly changing relationships between nations create new challenges and opportunities. Scientific efforts and space research must contribute to our ability to succeed in a vigorous economic and technological international competition.
- Domestic needs compete with scientific research in space and with the space program and force the nation to choose between research opportunities and other endeavors. Thus a focused and compelling space research agenda that clarifies the value and increases the productivity of both space research and the space program must be formulated.
- Public demand for accountability and for effective use of available resources is increasing. Space research and the space program must be conducted in accord with operating principles that will ensure that objectives are attained effectively. We must distinguish between initiatives in space that contribute to scientific understanding and those that are really aimed at nonscientific public purposes.
- There is widespread concern that our educational systems are not adequately preparing our citizens to participate effectively in an increasingly technological and competitive world. Success in space research can stimulate the curiosity of all young Americans and motivate some to choose careers in science, engineering, and technology disciplines. A vigorous space science program will provide information that interests, and perhaps enlightens, a national audience.
- Opportunities for international collaboration in space research are increasing. They are attractive because of the increasing complexity and cost of acquiring knowledge. But sharing the costs of space research with others cannot alone justify international collaboration; rather, collaboration should be undertaken in space research only to enhance scientific achievement.

OPERATING PRINCIPLES

Space research and the space program must be managed according to operating principles that will ensure that resources are used effectively and that objectives are attained. The following principles are derived from our 30 years of experience in space research; adhering to them will enhance the acquisition of information and knowledge and facilitate the response of space research and the space program to today's imperatives.

- *Enhance the human resource base.* The community of working scientists and students should be maintained and invigorated to strengthen the national scientific enterprise.
- *Acknowledge that choices must be made.* Science raises more intriguing questions than can be answered or even addressed. Thus we should recognize that choices must be made.
- *Capitalize on opportunities.* Special opportunities to perform good research are sometimes offered by technological developments or demands for applications. Wise investments in technological development will create such opportunities, sometimes in unexpected ways.
- *Capitalize on investments.* Having chosen to start valuable projects, we should insist on finishing them, in satisfactory, cost-effective ways. We need to understand better the direct and indirect costs of abandoning projects already begun.
- *Increase program control by principals.* Making principal investigators responsible for quality and giving scientists an increased role in program management offer potentially large benefits.
- *Secure access to space by diverse means.* Access to space through a variety of means appropriate to particular research missions is a recognized requirement of a vital space program.

THE RATIONALE FOR SETTING PRIORITIES

Priorities are needed at several levels within the national scientific enterprise, within the space program, and within space research because the success of science has created a wealth of opportunities for initiatives. Some initiatives will contribute more to scientific knowledge than others, some will enhance national economic and technological vitality, some will advance important applications of information from space, and some will assist in resolving important policy issues. An orderly process is needed to make the necessary choices.

Chapter 2 illustrates the broad range of future prospects for space research that includes large and small missions, projects in different fields, and the need to support both mature fields and untested ideas. Developing priorities for scientific research in space requires a sophisticated approach because it is not possible to rank all scientific research activities in a single list. Any priority scheme should be multidimensional in nature, with certain classes of activities given higher priority than others. There are a number of important criteria: the value of an initiative to science, potential social benefits, costs and readiness to perform it, and the probability of success. A priority scheme should provide for balance and flexibility in the program and for the maintenance of essential, ongoing activities.

Arguments for Setting Priorities

There are two principal arguments in favor of the recommendation of an agenda for space research by the scientific community:

- *Consensus is politically compelling.* An agenda for scientific research in space created and supported by the community would be persuasive. If scientists demonstrate that their agenda responds to scientific imperatives and to national needs, they can argue effectively for an adequate share of resources and for an orderly progression through the suite of initiatives endorsed by the community.
- *If scientists will not act, then others will.* If scientists cannot, or will not, recommend priorities, then others whose goals may differ from those of the scientific community will take the stage and make the decisions. None of the reasons scientists cite for eschewing the strenuous work of reaching consensus prevent federal officials or congressional representatives from making the necessary choices.

Addressing the Arguments Against Setting Priorities

A number of arguments against recommending priorities are sometimes offered by scientists. Some of them are listed below, with explanations as to why the task group does not find them compelling:

- *There will be losers.* Indeed there will be, but there are losers now. In fact, some who now enter the priority-setting process lose for reasons unrelated to the quality of the science. It would seem preferable that the community of scientists help to determine the winners.
- *Recommending priorities is too difficult, too contentious.* Recommending priorities is difficult but can be accomplished through a formal process in which competing initiatives are judged uniformly according to explicit criteria. If scientists find it too difficult to create a recommended program for space research, then, as said above, others will do it for them.
- *The community will not be able to maintain consensus.* Scientists loyal to initiatives not receiving strong recommendations may tend to subvert the process, it is argued, by lobbying for special favor. They would be better advised to develop more exciting initiatives. This argument and the two above combine to make a fourth:
- *Setting priorities will be counterproductive because the community will tear itself apart.* Moreover, the argument goes, at present the losers' rancor is directed at officials outside the community; if the community sets priorities, then the rancor will be turned inward. In essence, this is an argument that the science community is too immature to govern itself. The task group believes the community can behave responsibly and that its best interests will be served by doing so.
- *The low-priority initiatives will not be done.* The argument is that policy makers will take advantage of any list of priorities by eliminating the low-priority activities. That is precisely the reason priorities are recommended. It certainly seems preferable to abandon low-priority activities rather than to starve those with high priority.
- *Scientists cannot make political judgments.* Once scientifically meritorious proposals are put forward, this argument goes, the judgments about relative social benefits and the relevance to national needs are beyond the purview

of scientists. But the task group believes that in arguing for initiatives, scientists should be sensitive to national goals and political realities. Because scientists expect support from the public, they should be able to explain why some initiatives better serve public purposes.

Priorities have been successfully set by scientists in a number of contexts. For example, NASA's Office of Space Science and Applications (OSSA) has adopted a structured approach to the assignment of priorities using the priority recommendations of a scientific advisory committee. The result is a program in which annual budget requests are made in the context of a formal five-year plan. Clarifying the components of the program and specifically setting priorities among initiatives appear to have reduced uncertainty and divisiveness in the space research community, strengthened space research, and made the program more attractive to the policy makers who provide the resources for it.

CONCLUSION AND RECOMMENDATIONS

Space research operates within the vision that governs the overall civilian space program. The task group concludes that emphasizing the acquisition and processing of observations and information and the conversion of this information into knowledge and understanding will simultaneously advance science and contribute effectively to national economic and technological vitality. Even with such a vision, the need to determine priorities among the various initiatives is inevitable.

For these reasons the task group makes the following recommendations:

- Development of new knowledge and enhanced understanding of the physical world and our interactions with it should be emphasized as the principal objective of space research and as a key motivation for the space program.
- Acquisition and effective management of information derived from space should be a primary objective of our national activities in space. Concentrating on innovation in information management will produce benefits beyond space research.
- The requirements of space research itself should determine policy and programmatic decisions in space research and in the support of space research by the civil space program.

Finally, the task group recommends that the Space Studies Board proceed to the next phase of the Priorities in Space Research study and thereby develop a methodology for assessing priorities for scientific research in space. Such an assessment procedure is possible, and its application will allow the establishment of priorities in space research that will benefit science, the U.S. civil space program, and the nation. The members of the scientific community conducting research in space have a responsibility to the public to undertake this task.

3.2 Toward a Microgravity Research Strategy

A Report of the Committee on Microgravity Research[1]

INTRODUCTION

As part of a self-assessment and subsequent reorganization in 1989, the Space Studies Board (SSB) created a new standing committee—the Committee on Microgravity Research (CMGR). The formation of the committee was due, in part, to the dissolution of the National Research Council's (NRC's) Space Applications Board, which, until 1988, held the NRC's advisory responsibility for microgravity issues.

Over the course of the past 20 years, the Space Studies Board has, through its standing discipline committees, developed and published a series of research strategies for each of the major space research disciplines. These strategies are meant to serve as guides for the National Aeronautics and Space Administration (NASA) in planning its space research program. As one of its charges, the CMGR was asked by the SSB "... to conduct a study on the maturity and state of readiness of the field for the development of a comprehensive long-range research strategy."

In this report to the SSB, the CMGR finds that the various subdisciplines of the field are heterogeneous in both their nature and state of maturity. This is reflected in Appendixes A to F, which briefly discuss the status, accomplishments, and prospects and opportunities for each microgravity research subdiscipline. Notwithstanding this inherent heterogeneity, the CMGR concludes that the field as a whole would benefit from the formulation of a long-range research strategy and that such a strategy should be developed as soon as possible.

NATURE OF THE FIELD

Microgravity research encompasses scientific investigation conducted in a gravitational field (or equivalent acceleration with respect to an inertial frame) that is a small fraction of the gravitational acceleration on Earth. The role of gravity in physical phenomena is uniquely important in a limited set of circumstances, including the following:

- As a driving force for convection in fluids,
- As a driving force for phase separation,
- As a force that helps to determine the free surface morphology of fluids,
- Near a critical point,
- In the presence of very weak binding forces,
- In the presence of very large masses or for very long times, and
- In structural members or over large distances.

To date, most microgravity experiments have been focused on exploring the first two roles above. These experiments have included studies of crystal growth in fluids, fundamental phenomena in crystal growth, convection phenomena, measurement of the transport properties of fluids, combustion phenomena, fire safety aboard spacecraft, and immiscible alloys and multiphase solids.

STATUS OF THE FIELD

Between 1989 and 1991, the CMGR reviewed the status of microgravity research, the activities of NASA's Microgravity Science and Applications Division, and previous studies such as *Materials Processing in Space*,[2] *Microgravity Science and Applications*,[3] *Review of Microgravity Science and Applications Flight Programs—*

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[1] "Summary and Recommendations" reprinted from *Toward a Microgravity Research Strategy*, National Academy Press, Washington, D.C., 1992, pp. 1-6.

[2] Committee on Scientific and Technological Aspects of Materials Processing in Space, Space Applications Board. 1978. *Materials Processing in Space*. National Academy of Sciences, Washington, D.C.

[3] Solid State Sciences Committee, Board on Physics and Astronomy. 1986. *Microgravity Science and Applications: Report on a Workshop*. National Academy Press, Washington, D.C.

January-March 1987,[4] and *Fluid Sciences and Materials Science in Space—a European Perspective*.[5] Based on this review, the CMGR reached the following conclusions.

Fluids, interfaces, and transport; metals and alloys; and combustion science are more developed than the other subdisciplines of the field. The biological sciences category shows promise in the area of protein crystal growth, but little in other aspects such as electrophoresis. Current research holds out little hope for explaining why protein crystals grow differently in space or how to exploit the differences. Excellent—although only a few—experiments are planned in the subdiscipline of fundamental processes. Recommendations for future experiments in this direction are more likely to be derived from unsolicited proposals than from Announcements of Opportunity (AOs) issued by NASA. Research in the area of electronic materials has concentrated on bulk materials thus far. There is some indication that these experiments will produce information of scientific importance. However, concentration on bulk materials is contrary to the mainstream of the field, which emphasizes research on the properties of thin films deposited on substrates rather than research on electronic properties. Current research on the qualities of bulk crystals (when used as substrates) suggests that such crystals may hold some practical value. The subdiscipline of glasses and ceramics is relatively undeveloped at present; some research in this area overlaps with research in metals and alloys.

It should be recognized that microgravity research is a relatively new and laboratory-intensive field that requires frequent access to space. So far, progress has been limited considerably by the paucity of flight opportunities.

THE CONDUCT OF MICROGRAVITY RESEARCH

Microgravity research must be performed in an environment far from Earth and, therefore, is largely inaccessible. In addition, it is extremely expensive, both in terms of the initial investment and in operating costs, particularly when humans are involved.

The conduct of microgravity research requires the development of scientific equipment that is capable of withstanding the stresses of launch and reentry and of functioning reliably and safely in space. The interaction of users with this equipment is quite different from their interaction with other space instruments. Often, the users of microgravity equipment must change experimental parameters from run to run of an experiment. A more efficient approach to designing and building equipment would be to provide instrumentation that is specific to the experiment or class of experiments and that is designed and built in close cooperation with the principal investigator(s). This would be a departure from current practice, in which equipment is developed for a broad population of users.

Microgravity experiments can be carried out in a variety of modes, ranging from continuous human intervention to full automation. An optimum microgravity research program would use a mixture of modes, depending on the set of experiments to be performed, the state of the technology, and cost-effectiveness. Some microgravity experiments require a manned, space-based laboratory (such as a space station), while others can be done well or better, and at a much lower cost, by other means such as in satellites, rockets, and drop towers.

A wide range of facilities—from ground-based drop tubes to the complex facilities of the Shuttle-based Spacelab—can provide microgravity conditions. An experimenter's choice of facility should be based on specific research needs as well as cost.

CONCLUSIONS AND RECOMMENDATIONS

Development of a Research Strategy

The Committee on Microgravity Research recommends that a long-term research strategy, such as that developed by the Space Studies Board's other discipline committees, be developed for microgravity science. In addition to defining the overall goals of the microgravity science field and summarizing the current knowledge of its subdisciplines, this strategy should identify the fundamental questions that need to be addressed and the scientific

[4] Review Committee, J. Robert Schrieffer, chairman. 1987. *Review of Microgravity Science and Applications Flight Programs—January-March 1987*. Universities Space Research Association, Washington, D.C.

[5] European Space Agency. 1987. *Fluid Sciences and Materials Science in Space—a European Perspective*. Springer-Verlag.

community's ability to address them. Consideration should be given to all modes of doing this type of research, with attention to maximizing experimental return and minimizing cost. The primary objectives defined should be ranked in order of priority and should be accompanied by the criteria used to determine their priority. Critical components necessary to support a successful microgravity research program should be described and appropriate measurement indicators developed.

Microgravity Research Versus Materials Processing

It should be recognized that, to date, no examples have been found of materials that are worthy of manufacture in space. Unless and until such examples are found, space manufacturing of products to be used on Earth should be deemphasized as a reason for undertaking microgravity research. The descriptor "materials processing" is misleading and should be eliminated. The CMGR recommends that "microgravity research" be used instead. The main rationale for the microgravity research program should be to improve our fundamental scientific and technological knowledge base, particularly in areas that are likely to lead to improvements in processing and manufacturing on Earth. A secondary rationale should be to develop the technologies for handling materials in space and possibly for processing materials to be used in space.

Subdivisions for Microgravity Research

Microgravity research encompasses a wide range of subdisciplines. NASA's Microgravity Science and Applications Division and its advisory groups are currently divided into seven "disciplines": biological sciences; combustion science; electronic materials; fluids, interfaces, and transport; fundamental processes; glasses and ceramics; and metals and alloys.

After careful consideration, the Committee on Microgravity Research has concluded that the current subdivisions of microgravity science should be revised. The CMGR recommends that microgravity research be reorganized into six categories that reflect future opportunities more realistically, including:

- Biological science and technology,
- Combustion,
- Fluid science,
- Fundamental phenomena,
- Materials, and
- Processing science and technology.

Conduct and Support of the Research and Analysis Program

A thorough program of ground-based research should precede and follow every microgravity flight. When exemplary materials are produced in microgravity, attempts should be made to replicate them using ground-based research. In addition, much more effort should be made to model phenomena suggested by microgravity observations.

Research projects include both focused opportunities advertised through AOs issued by NASA and unsolicited proposals submitted to NASA. The research and analysis program in NASA's Microgravity Science and Applications Division consists of the ground-based research needed to provide the context of knowledge from which the flight program originates as well as the infrastructure required to analyze microgravity experiments in a broader context. If microgravity research is to develop into a mature field, the current research program should be reconstituted and refocused in order to improve its health and to provide new opportunities. The CMGR recommends that NASA apply a set of value criteria and measurement indicators to define the research and analysis program more clearly. These value criteria and indicators should be compared with other areas of physical and chemical sciences to calibrate funding levels with research output over a reasonable period of time (such as three years).

If research of higher quality and wider diversity is to be incorporated into the microgravity research program, it is imperative that the research and analysis budget be a larger fraction of the total microgravity budget. The CMGR recommends that the funding level for research and analysis in microgravity science be established as a fixed percentage of the total program of NASA's Microgravity Science and Applications Division in order to build a strong scientific base for future experiments.

Content of the Program and Facilities

Materials employed in microgravity environments should be characterized thoroughly before and after flight. The thermophysical data needed to interpret experiments should be measured as a part of the program if they are not available in the literature. Contemporary interest in electronic materials focuses on thin films. Bulk electronic materials are of secondary importance and should be studied in microgravity only to the extent that they will yield fundamental knowledge about processing.

When promising results have been obtained, experiments should be repeated to examine their reproducibility; in particular, experiments should be designed and conducted to learn why microgravity makes a measurable difference. Experiments should be analyzed and classified according to their minimum facility requirements so that they can be carried out in the most cost-effective manner. The committee recommends that a concerted effort also be made to classify experiments according to their minimum needs in order that the most cost-effective access to reduced gravity will be used. Equipment to accomplish specific experiments should be designed and built in close cooperation with the principal investigator(s). The acceleration vector environment must be measured accurately, locally, frequently, and synchronously with every experiment. These data should be provided to the principal investigators immediately. Whenever exemplary materials are produced in microgravity, considerable effort should be exerted to replicate them in ground-based research.

Commercial Programs

In addition to the activities financed by NASA's Microgravity Science and Applications Division, NASA funds commercial microgravity research through its Office of Commercial Programs. This office provides incentives for space experiments and, in cooperation with industry, has established centers for the commercial development of space (CCDS) at several universities. Started in 1986, these centers were given five years in which to become independent through increased industrial funding. The CMGR recommends that a thorough technical review of the centers for commercial development of space be conducted to determine the quality of their activities and to ascertain to what degree their original mission has been accomplished.

3.3 Biological Contamination of Mars: Issues and Recommendations

Report of the Task Group on Planetary Protection[1]

INTRODUCTION

Task, Approach, and Scope of Report

Whenever Earth-originating spacecraft intrude on the atmosphere or surface of other solar system bodies or return to Earth from one of these bodies, there is a risk of contamination by foreign substances or organisms. In the case of in situ exploration of other bodies, a major concern is disruption of scientific findings by imported material. In the case of back contamination (return to Earth of extraterrestrial material), there is concern over the possible release into the biosphere of potentially harmful organisms or substances.

Since 1967, a policy of planetary protection has been in place in order to control contamination of planets by terrestrial microorganisms and organic constituents during planetary missions. In the United States, the policy is implemented by the National Aeronautics and Space Administration (NASA). It is accepted as official policy by the Committee on Space Research (COSPAR) of the International Council of Scientific Unions. The policy lays out a framework of specific planetary protection guidelines for implementing procedures for future missions. Through COSPAR, review and analysis of the policy have been ongoing and have resulted in periodic revisions in light of new information obtained from planetary exploration.[2]

In addition, the United States is a signatory to an international treaty that declares in part that "States Parties to the treaty shall pursue studies of outer space . . . so as to avoid their harmful contamination and also adverse changes in the environment of the Earth. . . ."[3]

The Space Studies Board (SSB) of the National Research Council has served as NASA's primary advisor concerning planetary protection (or quarantine) for many years. The board, through its Committee on Planetary Biology and Chemical Evolution, has published a number of reports and letters concerning planetary protection (or quarantine) in response to NASA requests.[4] Most recently, NASA's planetary protection officer requested that, prior to the 1992 COSPAR meeting, the board make recommendations regarding planetary protection policy for upcoming Mars missions (Appendix A). In response to this request, the board formed the ad hoc Task Group on Planetary Protection, made up of planetary scientists, biochemists, ecologists, and microbiologists who specialize in studying life in extreme environments such as the polar regions and deep oceans and lakes (Appendix B). The task group hosted

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[1] "Summary and Recommendations" reprinted from *Biological Contamination of Mars: Issues and Recommendations*, National Academy Press, Washington, D.C., 1992, pp. 1-12.

[2] DeVincenzi, D.L., and P.D. Stabekis. 1984. "Revised Planetary Protection Policy for Solar System Exploration." *Adv. Space Res.* 4:291-295; also, DeVincenzi, D.L. 1990. "Planetary Protection Issues and the Future Exploration of Mars." *Adv. Space Res.* December preprint.

[3] United Nations. 1967. *Treaty on Principles Governing the Activities of States in the Exploration and Use of Outer Space, Including the Moon and Other Celestial Bodies.* U.N. Document No. 6347, January.

[4] Space Science Board, 1967, "Study on the Biological Quarantine of Venus," report of an ad hoc panel, January 9, National Academy of Sciences, Washington, D.C.; Space Science Board, 1970, "Review of Sterilization Parameter Probability of Growth (P_g)," report of an ad hoc review group, July 16-17, National Academy of Sciences, Washington, D.C.; Space Science Board, 1976, "On Contamination of the Outer Planets by Earth Organisms," report of the Ad Hoc Committee on Biological Contamination of Outer Planets and Satellites, Panel on Exobiology, March 20, National Academy of Sciences, Washington, D.C.; Space Science Board, 1976, "Recommendations on Quarantine Policy for Uranus, Neptune, and Titan," report of the Panel on Exobiology, May 24, National Academy of Sciences, Washington, D.C.; Space Science Board, National Research Council, 1977, *Post-Viking Biological Investigations of Mars*, Committee on Planetary Biology and Chemical Evolution, National Academy of Sciences, Washington, D.C.; Space Science Board, National Research Council, 1978, *Recommendations on Quarantine Policy for Mars, Jupiter, Saturn, Uranus, Neptune, and Titan*, Committee on Planetary Biology and Chemical Evolution, National Academy of Sciences, Washington, D.C.; letter report from SSB Chairman Thomas Donahue to NASA Office of Space Science and Applications Associate Administrator Burton I. Edelson regarding planetary protection policy, November 22, 1985; letter report from Committee on Planetary Biology and Chemical Evolution to Arnauld E. Nicogossian, director, Life Sciences Division, NASA, regarding the planetary protection categorization of the Comet Rendezvous-Asteroid Flyby mission, May 16, 1986; and letter report from Committee on Planetary Biology and Chemical Evolution to John D. Rummel, chief, Planetary Quarantine Program, Office of Space Science and Applications, NASA, regarding a formal recommendation on planetary protection categorization of the Comet Rendezvous-Asteroid Flyby mission and the Titan-Cassini mission, July 6, 1988.

a workshop in September 1991 at which extensive briefings on planned and contemplated Mars missions and the many aspects of Mars science and survival of Earth organisms were reviewed and discussed in detail (Appendix C). Scientists from Europe and the former USSR made presentations concerning their current views and approaches to planetary protection. These presentations and discussions, along with a reassessment of the SSB's 1978 report, *Recommendations on Quarantine Policy for Mars, Jupiter, Saturn, Uranus, Neptune, and Titan* (excerpted in Appendix D), form the basis for this report. Additional information considered by the task group is given in Appendix E.

In keeping with NASA's request, the task group focused on making recommendations concerning the protection of Mars from forward contamination (i.e., contamination of the martian environment by terrestrial organisms) during upcoming missions by both the United States and the former Soviet Union. In so doing, it distinguished between missions whose goals include reconnaissance and measurement and those that specifically include experiments to detect life. The task group also discussed what additional knowledge will be needed in order to assure that future recommendations regarding contamination of Earth from Mars (back contamination) might be made with a higher degree of certainty than is now possible.

Following a short introduction to the rationale underlying planetary exploration (Chapter 1) is a brief summary of approved and contemplated missions to Mars (Chapter 2). Chapter 3 briefly reviews the state of knowledge in several areas pertinent to the problem of planetary protection, including chemical and physical properties of Mars, and Chapter 4 discusses the limits of life on Earth and the abilities of known terrestrial organisms to withstand extreme environmental conditions, as well as new approaches to detecting life forms. Chapter 5 includes a review and comments—made in light of current knowledge—on the recommendations made in *Recommendations on Quarantine Policy for Mars, Jupiter, Saturn, Uranus, Neptune, and Titan.* Updates to the recommendations made in 1978 are also given in Chapter 5. Chapter 6 gives additional recommendations concerning collection of essential data, spacecraft sterilization and bioburden assessment, and future research, as well as legal and societal issues and NASA's overall planetary protection program.

Background

Understanding the origin and evolution of life has been an important goal of NASA; studies in this area generate some of the more interesting scientific questions for all mankind. One promising approach to understanding life's origins is that of searching for life elsewhere, primarily on other planets, where physical, hydrological, and geochemical properties might favor (or might have favored in the past) the existence of replicating biotic systems like those found on Earth. Historically, Mars has been the planet of choice for understanding life's origins.

With the technological advances that accompanied the advent of spacecraft exploration, our ability to conduct detailed studies of planets in the solar system improved dramatically. As our knowledge of present conditions on the surface of Mars has increased, there has been a concomitant decrease in any expectation that life as we know it could exist on the surface of the planet. At the same time, it is important to remember that (1) Viking lander sites have not been representative of the entire planet and (2) the early state of Mars seems to have differed quite markedly from its present state and may have been characterized by the presence of abundant liquid water and a more substantial atmosphere. Future life-detection missions to Mars must include investigation of other more biologically relevant, desirable sites where evidence of the survival of either molecular or morphologically preserved cells or cell components may exist.

As in the past, it is necessary to continue to take precautions to ensure planetary protection, both from forward and back contamination. With respect to forward contamination, NASA's historic concern has been to preserve pristine conditions on the planets for future experiments with biological and organic constituents that might lead to insights concerning the origin and evolution of life in the cosmos. Knowledge has increased substantially since the Viking mission. Recommendations for planetary protection that guided the Viking mission may not be relevant to missions being flown today or to those planned for the future. As more information is acquired about a given extraterrestrial body, assessment of the amount of planetary protection needed to protect that body from contamination should change accordingly. The process must be iterative and must allow for altering the techniques used to ensure protection as we learn more about planetary conditions and the probability of contamination.

FUTURE MISSIONS

At this time, there are two approved missions to Mars: the U.S. Mars Observer mission to be launched in October 1992 and the Soviet Mars 94/96 mission. Both NASA and the European Space Agency (ESA) are studying a network mission that involves placing numerous small stations on the surface of the planet. In addition, both the United States and the former Soviet Union have been studying various rover and sample return missions for some time. These missions, which will gradually improve our knowledge of the environmental parameters of Mars and enhance our ability to select and protect appropriate landing sites, are discussed in detail in Chapter 2.

SURFACE ENVIRONMENT OF MARS

Despite an incomplete understanding of the surface environment of Mars, it is generally agreed that conditions are extremely inhospitable to terrestrial life. Various aspects of the surface environment have relevance to the issue of forward contamination, including both growth on Mars of organisms from Earth and the lifetime of bioorganic matter deposited on the martian surface. Chapter 3 of this report reviews the state of knowledge regarding the martian surface, including its chemistry, solar radiation flux, temperature, water, volcanism, and past climate conditions.

LIMITS OF LIFE ON EARTH: EXPANSION OF THE MICROBIAL WORLD AND DETECTION OF LIFE

Life in Extreme Environments

The Task Group on Planetary Protection assessed past reports and current views on the range of environmental conditions believed to exist on Mars and unanimously agreed that it is extremely unlikely that a terrestrial organism could grow on the surface of Mars. It is clear that the most extreme environments on Earth where organisms can replicate are considerably less extreme than the environments that are known to occur over most of the martian surface. Particularly important in this regard are the high levels of ultraviolet radiation, the thin atmosphere, the extremely low maximum temperatures, and the absence of liquid water on the surface.

Based on current knowledge of conditions on Earth that limit cell growth and on the best estimates of surface conditions on Mars, the task group concluded that no known terrestrial organisms could grow on the martian surface. However, this does not imply that life does not exist anywhere on the planet. There is far too little information to assess the possibility that life may exist in subsurface environments associated with hydrothermal activity or in selected microenvironments free from the harsh conditions previously mentioned, or to conclude that organisms resembling terrestrial life forms did not evolve on Mars.

The task group concentrated on the problem of forward contamination by intact cells or components of cells that could be detected by sophisticated molecular methods in future expeditions designed to look for evidence of extant or past life on Mars. Planning for present and future missions to Mars must include awareness of new results obtained from studies of extreme environments as well as the inevitable extension of the limits of environments where growth and survival can take place. Advances in understanding the microbiology of extreme environments have been accompanied by advances in the development of new methods and considerably more accurate and sensitive instruments for detecting the presence of life and life-related molecules and for identifying their evolutionary relatedness.

Nevertheless, it is not a straightforward matter to define the ranges of physical and chemical conditions on Earth in which organisms can grow, replicate, or survive for extended periods. During the 13 years since the SSB's last report on planetary protection, *Recommendations on Quarantine Policy for Mars, Jupiter, Saturn, Uranus, Neptune, and Titan*, bacteria have been detected or isolated from many of Earth's hostile environments—the dry, extremely cold subsurfaces and interiors of rocks in the dry valleys of the Antarctic, hot environments associated with submarine and terrestrial volcanoes and geothermal systems, and deep subsurface sediments and aquifers. Chapter 4 includes a review of these organisms.

Life Detection and Bioburden Determination for Planetary Protection

Techniques for assessing the existence of microorganisms have advanced dramatically since pre-Viking days. These advances will have a strong impact both on bioburden assessment procedures and on future life-detection experiments. New methods have been developed with increasingly greater sensitivity and specificity. The task group strongly recommends that efforts be made to explore current analytical methods for use in bioburden assessment and inventory procedures before spacecraft assembly and launch.

In addition to epifluorescent microscopic techniques for directly counting viable cells, many other new methods have been developed, such as the polymerase chain reaction, allowing greatly increased sensitivity of detection by enzymatically amplifying specific biomarkers of even a single cell to detectable levels. The appeal of these techniques is their extreme sensitivity. In many cases, single cells can be detected and identified with confidence.

ASSESSMENT OF THE 1978 REPORT

Review

Recommendations on Quarantine Policy for Mars, Jupiter, Saturn, Uranus, Neptune, and Titan, the 1978 report by the then Space Science Board's Committee on Planetary Biology and Chemical Evolution, established a quarantine policy for exploratory, one-way missions to Mars, Jupiter, Saturn, Uranus, Neptune, and Titan planned for 1974 to 1994. The task group's assessment of this report is limited to an evaluation of information and past recommendations concerning Mars. After the 1978 report was issued, NASA began to look for ways to simplify planetary protection procedures as they applied to particular upcoming planetary missions, and to minimize the use of mathematical models.

Prior to the 1978 report, the criteria used for determining categories of planetary contamination were those established by international agreement through COSPAR. They stipulated that the probability of contamination (P_c) should be less than 0.001 for each planet. Considerable uncertainty was engendered by this probabilistic approach to planetary protection. Concern related to this point has been expressed over the years by virtually every group that has analyzed the problem, and indeed by NASA. Although the probability of depositing a microbe or some organic material indicative of life is very high (microbes and organic contaminants have almost certainly been deposited by past missions), our expectations regarding the likelihood of permanent contamination as a result of microbial growth (expressed as the probability of growth, P_g) have been steadily reduced as we have learned more about Mars.

The NASA studies that followed the 1978 report culminated in a 1984 report to COSPAR that greatly deemphasized the probabilistic approach and introduced the concept of target planet and mission-type categories.[5] This approach, which is reviewed in Chapter 5, directly reflects the degree of concern for a given planet, in the context of a particular type of mission.

Recommendations of the Task Group

The task group views the problem of forward contamination as separable into two principal issues: (1) the potential for growth of terrestrial organisms on Mars and (2) the importation of terrestrial organic contaminants, living or dead, in amounts sufficient to compromise the search for evidence of past or present life on Mars itself.

The guidelines concerning probabilities of growth (P_g) issued by the Space Science Board in its 1978 report were recently reassessed in a 1991 NASA report.[6] Comments and estimates made by the participants illustrate a consensus that the P_g values for terrestrial organisms on Mars are probably lower than the 1978 estimates. However, this observation does not alter the case as far as contamination of a possible past or extant martian biosphere is concerned. Prudence dictates that bioload reduction on all lander missions to Mars must continue to be seriously addressed. The issue of spacecraft cleanliness is particularly crucial when life-detection experiments are included in the scientific payload.

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[5] See DeVincenzi and Stabekis, 1984.

[6] Klein, H.P. 1991. *Planetary Protection Issues for the MESUR Mission: Probability of Growth (P_g)*, NASA conference publication, NASA Ames Research Center, Moffett Field, Calif.

The deliberations of the task group were greatly aided by the MESUR mission workshop that resulted in the above-mentioned 1991 report. That report, together with the comprehensive briefings given by experts on relevant matters, led the task group to concur unanimously with the following conclusion from the MESUR workshop:

> Forward contamination, solely defined as contamination of the martian environment by growth of terrestrial organisms that have potential for growth on Mars, is not a significant hazard. However, forward contamination more broadly defined to include contamination by terrestrial organic matter associated with intact cells or cell components is a significant threat to interpretation of results of in situ experiments specifically designed to search for evidence of extant or fossil martian microorganisms.

Based on the MESUR group's consensus and the task group's agreement with it, the task group makes the following recommendations for control of forward contamination, each tied to specific mission objectives.

- Landers carrying instrumentation for in situ investigation of extant martian life should be subject to at least Viking-level sterilization procedures. Specific methods for sterilization are to be determined. Viking technology may be adequate, but requirements will undoubtedly be driven by the nature and sensitivity of the particular experiments. The objective of this requirement is the reduction, to the greatest feasible extent, of contamination by terrestrial organic matter and/or microorganisms deposited at the landing site.
- Spacecraft (including orbiters) without biological experiments should be subject to at least Viking-level presterilization procedures—such as clean-room assembly and cleaning of all components—for bioload reduction, but such spacecraft need not be sterilized. Table 1.1 in Chapter 1 summarizes Viking-level procedures, and Appendix E includes a detailed description of the procedures.

The task group sees little utility in further attempts to estimate actual probability-of-contamination values in various martian environmental regimes. In the absence of crucial data relating to the survivability and growth potential of terrestrial organisms on Mars, such exercises are purely subjective. The task group emphasizes that the philosophical intent underlying the 1978 report—to protect Mars from terrestrial contamination so as not to jeopardize future experiments aimed at detecting martian life—is still profoundly important.

ADDITIONAL RECOMMENDATIONS

Recommendations for Research

The task group strongly recommends that a sequence of unpiloted missions to Mars be undertaken well in advance of a piloted mission. Any future changes in recommendations to ensure planetary protection, especially for piloted or sample return missions, will depend on the acquisition of new data. With regard to these missions, the task group recommends that a broad spectrum of martian sites be examined, with emphasis on measurements that provide data most likely to contribute to models that provide for a better understanding of the probability of life on Mars and where best to go to find it.

Until such data are available, it will be impossible to make informed decisions concerning landings for in-depth biological study. Such data will also greatly affect the ability to make future decisions concerning the rigor required for spacecraft cleanliness and possible sterilization.

Location of martian lander sites should take into account our rudimentary but growing understanding of Mars and our extensive knowledge of the basic requirements of life. It is also important to consider the subsurface of Mars. Within a site, it may prove important to plan for data collection that probes below the readily accessible surface, in order to obtain information on subsurface environments. Microenvironments—whether on the surface or in isolated vents, cracks, or layers of the subsurface—may exist now or may once have existed at some time in the past. Properly designed experiments may be able to address the issue of spatial and (perhaps) temporal heterogeneity and its possible relationship to our ability to evaluate the biotic and abiotic status of a given site.

Collection of appropriate data should allow the scientific community to amend planetary protection policy recommendations for back contamination, perhaps resulting in recommendations similar to the alterations in procedures for assessing forward contamination suggested by this task group. The determination of current or inferred past geophysical conditions on Mars may help identify locations where life-detection missions should be sent.

Recommendations Regarding Assessment of Spacecraft Bioload

The task group's recommendation to reduce bioload on all spacecraft and to sterilize those spacecraft used in life-detection missions assumes the use of Viking procedures. However, the task group recommends that the Viking protocols for assessment of spacecraft bioloads be upgraded to include state-of-the-art methods for the determination of bioload. It is critical that methods for assessing bioload be compatible with methods used to detect life, with methods for both assessment and detection reflecting the same limits and sensitivity. Data on bioloads of Viking components and spacecraft are not relevant to current life-detection procedures. Modern methods of bioburden assessment should be developed for and applied to spacecraft destined for future Mars missions, especially those carrying in situ extant life-detection experiments. Although immediate use of these techniques is not a feasible goal, the development of the methodology in anticipation of future life-detection missions is absolutely essential.

Recommendations Concerning Other Issues

Piloted Versus Unpiloted Missions

Missions carrying humans to Mars will contaminate the planet. It is therefore critical that every attempt be made to obtain evidence of past and/or present life on Mars well before these missions occur. The issues of forward and back contamination have societal, legal, and international implications. These implications are serious, and they deserve discussion and attention.

Societal Issues

A substantial number of active national and international organizations are on the alert for environmental abuse. There is every reason to take seriously the concern (already expressed in some cases) about contamination of Mars and almost certainly about the issue of back contamination of Earth by martian samples. Although public concern over such issues is often sincere and productive, it at times becomes distorted and exaggerated in the media, leading to public misunderstanding and opposition. The task group recommends that NASA inform the public about current planetary protection plans and provide continuing updates concerning Mars exploration and sample return.

Legal Issues

There are also legal issues that must be addressed, involving international restrictions as well as federal, state, and local statutes that may come into play. There are currently no binding international agreements concerning forward or back contamination. The task group recommends as essential that efforts be made (1) to assess the legal limits (and implied liabilities) in existing legislation that relates to martian exploration and (2) to pursue the establishment of international standards that will safeguard the scientific integrity of research on Mars. Furthermore, the task group recommends that NASA make a strong effort to obtain international agreement for a planetary protection policy.

NASA Planetary Protection Program

Although a planetary protection officer currently exists at NASA, there is no budgeted program (as there was during the Viking Program) to implement needed planetary protection research, a public education program, examination of legal and international issues, and the like. The task group recommends that NASA redefine the responsibilities and authority of its planetary protection officer and provide sufficient resources to carry out the recommendations made in this report.

SUMMARY OF RECOMMENDATIONS

All of the recommendations put forward by the task group in this report are summarized below. Each is discussed further in the full report in the chapter(s) indicated.

1. Efforts should be made to adopt current molecular analytical methods for use in bioburden assessment and inventory procedures for spacecraft assembly and launch for future missions, and also to develop new methods for the same purposes (Chapters 4 and 5).

2. Landers carrying instrumentation for in situ investigation of extant martian life should be subject to at least Viking-level sterilization procedures. Specific methods for sterilization are to be determined; Viking technology may be adequate, but requirements will undoubtedly be driven by the nature and sensitivity of the particular experiments. The rationale for this requirement is the reduction, to the greatest feasible extent, of contamination by terrestrial organic matter that is deposited at the site by microorganisms or organic residues carried on the spacecraft (Chapter 5).

3. Spacecraft (including orbiters) without biological experiments should be subject to at least Viking-level presterilization procedures—such as clean-room assembly and cleaning of all components—for bioload reduction, but such spacecraft need not be sterilized (Chapter 5).

4. A sequence of unpiloted missions to Mars should be undertaken well in advance of a piloted mission (Chapter 6).

5. A broad spectrum of martian sites should be examined with emphasis on measurements that provide data most likely to contribute to a better understanding of the probability of life on Mars and where best to go to be able to detect it (Chapter 6).

6. The Viking protocols for assessment of spacecraft bioloads should be upgraded to include state-of-the-art methods for the determination of bioload (Chapter 6).

7. NASA should inform the public about current planetary protection plans and provide continuing updates concerning Mars exploration and sample return (Chapter 6).

8. It is essential to assess the legal limits (and implied liabilities) in existing legislation that relates to martian exploration and to pursue the establishment of international standards that will safeguard the scientific integrity of research on Mars (Chapter 6).

9. NASA should make a strong effort to obtain international agreement for a planetary protection policy (Chapter 6).

10. NASA should redefine the responsibilities and authority of its planetary protection officer and provide sufficient resources to carry out the above recommendations (Chapter 6).

4
Letter Reports

During 1992, the Space Studies Board and its committees released ten letter reports, which this section presents in full in chronological order of release. Letter reports generally arise out of deliberations by a discipline committee in connection with its oversight responsibility. These reports, which document the consensus of the authoring committee, are submitted to the Board for approval at its next meeting. The members of the Board review the arguments and recommendations contained in the committee submission. Often, the Board writes a cover letter that may address, from a broader perspective, issues raised in the committee submission. In these cases, the final package is structured as a cover letter that summarizes the conclusions of the committee in the context of these broader issues and that is signed by the chairs of both the Board and the authoring committee, and the committee's scientific assessment, which is provided as an attachment. Examples are the flight program reviews of the Committee on Planetary and Lunar Exploration. In other cases, such as the 1992 statements on the space station and AXAF, the Board itself develops a recommendation and submits it to NRC review for approval and release.

In response to a request it received on September 26, 1991, the Committee on Planetary and Lunar Exploration assessed the congruence between NASA's Solar System Exploration Division's 1991 strategic plan document and previous advice of the committee. The letter (Section 4.1) discussing the committee's conclusions was approved and released on January 14. On March 30, the Board sent a letter (4.2) to NASA Administrator Richard Truly with a set of reports on Space Station Freedom, the baseline CRAF/Cassini mission, and AXAF. This package included a space station letter and a scientific assessment (4.3) addressed to Mr. Arnold Aldrich, Associate Administrator for Space Systems Development, and letters to Dr. Lennard Fisk, Associate Administrator for Space Science and Applications, on CRAF/Cassini (4.4) and AXAF (4.5). Following completion by the Committee on Earth Studies of a requested review of NOAA's strategic plan for polar-orbiting operational environmental satellites, the Board sent a preliminary assessment (4.6) on April 30 to Mr. Russell Koffler, NOAA Deputy Assistant Administrator for Satellite and Information Services.

Later, on August 20 and 21, the Board released to NASA a second set of reports dealing with several proposed planetary missions and with the future of the BEVALAC accelerator facility. Prepared by the Committee on Space Biology and Medicine, a letter (4.7) discussing the role of the Department of Energy's BEVALAC accelerator facility was sent to Energy Secretary James Watkins and to NASA Administrator Daniel Goldin. A letter and report (4.8) on the Office of Exploration's proposed robotic lunar precursor missions were sent to Associate Administrator Michael Griffin. A second letter and report (4.9) on the joint Strategic Defense Initiative Organization (SDIO)/NASA Clementine mission to the Moon and an asteroid were sent to SDIO Deputy for Technology, Dr. Simon Worden, and to NASA Director of the Solar System Exploration Division, Dr. Wesley Huntress. On October 19, the Board forwarded a letter and supporting COMPLEX assessment (4.10) on the restructured Cassini-only mission to Dr. Lennard Fisk, Associate Administrator for Space Science and Applications.

4.1 On the Solar System Exploration Division's 1991 Strategic Plan

The Committee on Planetary and Lunar Exploration sent the following letter to Dr. Wesley Huntress, Director of NASA's Solar System Exploration Division, on January 14, 1992.

At the September 26, 1991, meeting between the Space Studies Board committee chairs and NASA division managers, you requested that the Committee on Planetary and Lunar Exploration (COMPLEX) review the Solar System Exploration Division's 1991 strategic plan. The committee conducted that review at its October 28-29 meeting.

COMPLEX finds this plan to be clearly written, well articulated, scientifically rich, and broadly consistent with the recommended strategies of the Space Studies Board, where they exist. With regard to the exploration of the outer planets, however, COMPLEX has not yet established science strategies for the study of Neptune and Pluto. When the committee's last strategy for the outer planets was published in 1986,[1] too little was known about these planets to give reasoned advice. Therefore, the committee cannot comment on the congruence with previous recommendations in this area. Nevertheless, COMPLEX is now beginning a cross-cutting study to devise an integrated strategy for all planetary exploration and to recommend priorities for activities in the next decade. You can be assured that the merits of the study of Neptune and Pluto will receive significant discussion.

COMPLEX endorses the prominent emphasis given in the strategic plan to research and analysis as part of a program for planetary exploration. This emphasis is entirely consistent with the committee's past recommendations,[2] and we support your efforts to protect this important element.

COMPLEX has read with interest your preliminary description of the new Discovery Program for rapid, inexpensive missions. As noted in previous COMPLEX reports, the committee strongly endorses this philosophy.[3] COMPLEX anticipates reviewing your plans for implementing this new line of missions when the program reaches a sufficient level of definition.

If we can be of further assistance, do not hesitate to call on us.

Signed by
Larry W. Esposito
Chair, Committee on Planetary and Lunar Exploration

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[1] *A Strategy for Exploration of the Outer Planets: 1986-1996*, Committee on Planetary and Lunar Exploration, Space Science Board, National Academy Press, Washington, D.C., 1986.

[2] *Assessment of Solar System Exploration Programs: 1991*, Committee on Planetary and Lunar Exploration, Space Studies Board, National Academy Press, Washington, D.C., 1991, p. 31.

[3] Reference 2, pp. 30-31.

4.2 Letter to the NASA Administrator

The Space Studies Board sent the following letter to NASA Administrator Richard H. Truly on March 30, 1992. Included with it were the subsequent letters and assessments on the space station, CRAF/Cassini, and AXAF.

At its 105th meeting on February 26–28, 1992, the Space Studies Board was briefed by officials of NASA and of the Congressional Budget Office on NASA's budget request and the national budgetary outlook. The Board also heard a briefing by Space Station Freedom officials on the status of that program, and reviewed a scientific assessment of the CRAF (Comet Rendezvous - Asteroid Flyby) and Cassini (Titan Probe - Saturn Orbiter) missions prepared by a subpanel of the Board's Committee on Planetary and Lunar Exploration. Based on these briefings, the Space Studies Board has prepared and hereby submits assessments of the evolving Space Station Freedom design and of the CRAF/Cassini mission. A summary of past National Research Council recommendations regarding the Advanced X-ray Astrophysics Facility (AXAF), which is facing a restructuring in response to budget pressures, is also included. These assessments of individual programs are provided in the context of several broader concerns.

It is evident that some difficult programmatic choices have already been made by NASA and, possibly, more remain to be made by Congress. In such times, there is the potential for decisions that could cause serious unintentional damage to major national space research objectives. It is the Board's belief that both Congress and the Administration desire to maintain an effective space and aeronautics program that is responsive to the nation's needs. The four themes expressed in NASA's *Vision 21* plan are viable, but fragile, and the Board believes that the nation may be perilously close to forced decisions that will create an unbalanced NASA program.

The challenges posed by the Administration's budget proposal are clear. The Space Studies Board recognizes that as advisor to the civil space program, it bears responsibility to help evolve an effective management response to budget realities and to help incorporate new technical approaches enabling lower-cost missions. Such actions must be taken rapidly to preserve the positive attributes of a space program that has provided the nation with immense returns in science, technology, and pride of accomplishment.

From one perspective, the NASA program in space science and applications is vital and vigorous. Important missions have been launched in recent years and more are scheduled for 1992, providing a flow of valuable scientific information. From another perspective, however, the program faces a longer-term future in which scientific returns may be greatly diminished. Missions judged to be of high priority (including the Orbiting Solar Laboratory (OSL) and the Space Infrared Telescope Facility (SIRTF)) have been canceled or abandoned. Others (including the Earth Observing System (EOS), the Comet Rendezvous - Asteroid Flyby (CRAF)/Cassini missions, and the Advanced X-ray Astrophysics Facility (AXAF)) are experiencing continuing cost difficulties and are being reevaluated and possibly delayed, descoped or canceled. The run-out costs anticipated for key missions exceed realistic estimates of the funds that will be available in the years to come. We recognize that scientists, as others, are subject to the effects of large budget deficits and that research must compete with other national needs. We feel obligated, however, to articulate the consequences of budgetary decisions.

The Space Studies Board plans to continue its reassessment of elements of the space research program in light of existing budget constraints and to develop specific recommendations for the new NASA administrator.

Signed by
Louis J. Lanzerotti
Chair, Space Studies Board

4.3 On the Space Station Freedom Program

The Space Studies Board sent the following letter and attached assessment to Mr. Arnold D. Aldrich, NASA Associate Administrator for Space Systems Development, on March 30, 1992.

At its February 26-28, 1991, meeting at the Jet Propulsion Laboratory, the Space Studies Board was briefed by officials of the Space Station Freedom program on the results of the congressionally mandated restructuring activity. The restructured plan and design described at that briefing presented several areas of significant concern for research utilization of the space station. The Board articulated these concerns in a letter and attached statement addressed to Administrator Richard Truly on March 14, 1991. A copy of this letter and enclosure are appended for your convenient reference.

A year later, at its meeting on February 27, 1992, in Washington, D.C., the Board was given an update briefing by Mr. Richard Kohrs and other members of the Freedom program management team. The Board compared progress in the program to its previous advice and has summarized its assessment in the enclosed statement. The life sciences research program envisioned for Space Station Freedom is essential to achieving an understanding of the performance of humans in long-duration spaceflight. The enclosed statement notes that the inclusion of provisions for the centrifuge and related life sciences equipment in the baseline plan is a welcome development. The statement goes on, however, to elucidate several areas of continuing concern.

This past year has been marked by clear improvement in communications between the Space Station Freedom program and the Board; we look forward to continuing and expanding this dialogue in the coming year.

Signed by
Louis J. Lanzerotti
Chair, Space Studies Board

SPACE STUDIES BOARD ASSESSMENT OF THE SPACE STATION FREEDOM PROGRAM

The Space Studies Board was briefed by representatives of the Space Station Freedom program on February 27, 1992. This briefing and subsequent discussion focused on space station planning and design for science utilization.

In March 1991, the Board issued a statement[1] questioning the cost-effectiveness of the reconfigured Space Station Freedom for microgravity research and its adequacy for life sciences research. The statement recognized that there are national considerations other than scientific research for building a space station. It also noted, however, that a properly equipped and configured space station is pivotal to the conduct of the life sciences research needed in advance of long-duration human spaceflight. The Board is gratified that resources and planning for a 2.5-meter centrifuge and supporting equipment are now included in the Freedom baseline. According to this baseline, the centrifuge will be launched in an integrated node in late 1999. The Board was also pleased to learn that a life scientist, Dr. Robert Phillips, has been appointed as the next chief scientist of the Freedom program.

Both the Space Science Board[2] and the Advisory Committee on the Future of the U.S. Space Program[3] have recommended that life sciences research should be the primary scientific use for an Earth-orbiting space station. In this regard, the Board notes several areas of continuing concern. These include the following:

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[1] "Space Studies Board Position on Proposed Redesign of Space Station Freedom," Space Studies Board, Washington, D.C., March 14, 1991.

[2] "Space Science Board Assessment of the Scientific Value of a Space Station" and letter to NASA Administrator James Beggs, Space Science Board, Washington, D.C., September 9, 1983. *See also:* Space Studies Board, testimony to U.S. Senate Subcommittee on Science, Technology and Space, May 10, 1990; and *Toward a New Era in Space—Realigning Policies to New Realities—Recommendations for President-Elect George Bush*, Committee on Space Policy of the National Academy of Sciences and the National Academy of Engineering, National Academy Press, Washington, D.C., 1988.

[3] *Report of the Advisory Committee on the Future of the U.S. Space Program*, Superintendent of Documents, Government Printing Office, Washington, D.C., December 1990.

- As articulated in the Board's March 1991 statement: the small crew size at Permanently Manned Capability (four members, of which only two will be available to conduct research); uncertain adequacy of power, supporting equipment, and space; and the delay until the early 2000s in accomplishing life sciences research that is essential prior to long-duration human spaceflight;
- The vulnerability of the schedule for installation of the centrifuge and for the life sciences research enabled by it. Freedom system design and assembly planning should give higher priority to making the most direct and rapid progress possible toward establishing capability for life sciences research—essential to human space exploration goals—including installation of the 2.5-meter centrifuge, rather than to maintaining the schedule for station first element launch and subsequent Man-Tended Capability; and
- The tenuous programmatic linkage between Freedom program planning and human space exploration requirements, particularly in the area of life sciences research. NASA should establish a clear, long-term plan for Freedom's utilization for life sciences research. This planning should address the impact of microgravity and of psychosocial factors on humans in transit between Earth and Mars; the design, test, and critical evaluation of applicable mitigation measures; and the consequences of fractional gravity as a working environment both in transit and on the surfaces of the Moon and Mars.

4.4 On the CRAF/Cassini Mission

The Space Studies Board and its Committee on Planetary and Lunar Exploration sent the following letter and assessment to Dr. Lennard Fisk, NASA Associate Administrator for Space Science and Applications, on March 30, 1992.

At its meeting on February 18 and 19, 1992, a subpanel of the Space Studies Board's Committee on Planetary and Lunar Exploration (COMPLEX) chaired by Professor Peter H. Stone, of the Massachusetts Institute of Technology, carried out a detailed review of the CRAF (Comet Rendezvous - Asteroid Flyby) and Cassini (Titan Probe - Saturn Orbiter) missions. This review was part of COMPLEX's continuing advisory program to assess the responsiveness of NASA missions to science objectives given in COMPLEX's published strategies for exploring the solar system. The results of this review were presented to the Space Studies Board at its meeting on February 26-28 for consideration in the broad context of the status and outlook of the U.S. civil space research program.

COMPLEX's review was planned well before the release of the President's budget message on January 29. That message proposed cancellation of CRAF and called for a reassessment of the technical and schedule risks in the Cassini program. Nevertheless, COMPLEX proceeded with its review to assist those who must respond to the President's proposal. We recognize that scientists, as others, are subject to the effects of large budget deficits and that research must compete with other national needs. We feel obligated, however, to articulate the consequences of budgetary decisions. This letter summarizes the Board's overall assessment of the two missions and is accompanied by a summary providing COMPLEX's detailed scientific evaluation.

In brief, the Board recognizes that the current and near-term national budget environment severely constrains the conduct of the nation's space research program. Therefore, the Board recommends that NASA carefully reevaluate the Cassini spacecraft and instrument complement with the objective of ensuring the mission's prospects for adequate and stable funding leading to the scheduled 1997 launch, while retaining the maximum science content possible. This reevaluation should take into account cancellation of CRAF, if this mission is indeed canceled as proposed in the President's FY 93 budget message. Based on COMPLEX's evaluation, it is the strong recommendation of the Board that a scientifically responsive Cassini mission, reconfigured if necessary, proceed to development and launch on the present schedule.

The Board is dismayed by the proposed cancellation of the CRAF mission, which would be of great scientific merit even without the comet penetrator experiment. However, the Board recognizes that present and anticipated resources are not likely to be adequate to successfully undertake both missions and to meet the science objectives of both at this time.

The U.S. program of outer solar system exploration has brilliantly demonstrated American vision and technical mastery. The United States, alone, has undertaken and completed the initial reconnaissance of the major planets of the outer solar system, visiting in turn Jupiter, Saturn, Uranus, and Neptune and obtaining revolutionary data about these planets and their atmospheres, moons and rings, and plasma environments. The Board believes that a vigorous program of outer solar system exploration is an essential part of a national space exploration agenda. Because of the very long travel times to the outer solar system, seven or more years, it is important not to interrupt development of our next mission or delay its launch. The Saturn system, with its complex interacting system of magnetic fields, plasmas, rings, and moons, is an ideal laboratory for many of the physical processes believed to be important in the formation and present-day dynamics of our solar system and of planetary systems of other stars. It is for these reasons that the Board believes that high priority within the broad civil space agenda should be attached to the ongoing U.S. Saturn exploration program. We further believe that the Cassini mission should proceed without delay in order to benefit from the extremely favorable orientation of Saturn's rings at the spacecraft's projected arrival in 2004.

This review of the Cassini mission was COMPLEX's first since the initial selection of the instrument payload and has been completed prior to the final confirmation of these instruments. Thus, COMPLEX's conclusions are based on the current state of definition of the mission. Given the pending confirmation of the payload, and the programmatic changes that could result from the proposed cancellation of CRAF and technical reassessment of Cassini, COMPLEX plans to reexamine Cassini at a later time.

Signed by
Louis J. Lanzerotti
Chair, Space Studies Board

SCIENTIFIC ASSESSMENT OF THE CRAF AND CASSINI MISSIONS

Summary

At its meeting on February 18 and 19, 1992, a subpanel of the Space Studies Board's Committee on Planetary and Lunar Exploration (COMPLEX) chaired by Professor Peter H. Stone, of the Massachusetts Institute of Technology, carried out a detailed review of the CRAF (Comet Rendezvous - Asteroid Flyby) and Cassini (Saturn Orbiter - Titan Probe) missions. This review was part of COMPLEX's continuing advisory program to assess the responsiveness of NASA missions to science objectives given in COMPLEX's published strategies for exploring the solar system.

It is COMPLEX's opinion that Cassini is highly responsive to the scientific priorities set out in its report, *A Strategy for Exploration of the Outer Planets: 1986-1996.*[1] The instrument payload that has been tentatively selected, the mission plan that has been outlined, and the spacecraft that is being developed together provide an excellent opportunity to advance our understanding of Saturn and its satellites, rings, and magnetosphere. The Saturn system is unique within the solar system because of the wide variety of interactions—electrodynamical, hydrodynamical, and gravitational—among the system's different components. Improving our understanding of these interactions is important for developing better theories of evolution of the early solar system and of planetary and satellite systems in general. In addition, study of Titan's atmosphere is of high priority because it has a composition and chemistry that may be similar to Earth's early atmosphere. The Cassini mission as currently configured is extremely responsive to the objective of studying the Saturn system as a whole.

COMPLEX notes with concern that present budget constraints are jeopardizing all of the planetary program's large missions, including Cassini. The recent reconfiguration of the Earth Observing System into a series of small spacecraft might be thought to provide a guide for the achievement of science goals outside the context of large missions. Such an analogy is inappropriate for Cassini. The long travel times between Earth and the outer solar system require long-lived components, specialized power systems, and long-distance communications fundamentally different from those required for Earth-orbital missions. With current technology, any mission sent past the asteroid belt must be more than a Discovery-class mission. While intermediate-size missions (larger than Discovery class, but smaller than Cassini) could undoubtedly achieve some of COMPLEX's objectives for the Saturn system, they could not achieve many others. For example, studies of the interactions between the different components of the system, and concurrent coordinated observations of Titan's atmosphere by both the Huygens probe and by remote sensing instruments, require large suites of instruments that place heavy demands on the spacecraft's resources. Thus COMPLEX believes that the Cassini exploration of the Saturn system cannot be fully accomplished by reconfiguration into one or more small spacecraft.

COMPLEX views with dismay the proposal to cancel the currently approved CRAF mission. This mission is the outcome of many years of planning by numerous groups of distinguished scientists, NASA centers, and competitively selected scientific instrument teams. In proposing and planning CRAF, the research community fully recognized the importance of assessing priorities in choosing to pursue this major endeavor.

COMPLEX has long articulated the unique scientific opportunities provided by the in situ study of cometary nuclei, believed to be the best-preserved relics of the earliest history of our solar system. The report *Strategy for the Exploration of Primitive Solar System Bodies—Asteroids, Comets, and Meteoroids: 1980-1990*,[2] assigned highest priority to reconnaissance and initial exploration of comets, with special emphasis on the rendezvous mode planned for CRAF. This mode is essential for studying the sequence of events that occurs as a comet approaches and recedes from the sun. The CRAF mission has been developed in full accordance with the science objectives and recommendations of COMPLEX. In addition, the mission incorporates an excellent set of asteroid flybys, another high-priority recommendation of COMPLEX.

COMPLEX recognizes that budget constraints have forced significant changes in CRAF since its last review in July 1990. These changes were the deletion of the penetrator experiment (PENL) and of the Scanning Electron Microscope and Particle Analyzer (SEMPA) experiment, a launch delay, a change in the mission's targets, and an

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[1] *A Strategy for Exploration of the Outer Planets: 1986-1996*, Committee on Planetary and Lunar Exploration, Space Science Board, National Academy Press, Washington, D.C., 1986.

[2] *Strategy for the Exploration of Primitive Solar System Bodies—Asteroids, Comets, and Meteoroids: 1980-1990*, Committee on Planetary and Lunar Exploration, Space Science Board, National Academy of Sciences, Washington, D.C., 1980.

increase in the required lifetime of the mission. However, in COMPLEX's opinion, these changes do not invalidate its earlier judgments. CRAF remains a scientifically sound mission, responsive to COMPLEX's most important near-term priorities for the exploration of primitive solar system bodies. Cancellation of CRAF will not lessen the importance of these scientific objectives, which should be pursued at the earliest possible opportunity.

Cassini

COMPLEX's 1986 report, *A Strategy for Exploration of the Outer Planets: 1986-1996,* states that the highest priority for outer planet exploration in the next decade is intensive study of Saturn—the planet, satellites, rings, and magnetosphere—as a system. Specifically, the recommended exploration and intensive study of the Saturn system include the following objectives:

- Titan's atmosphere: Measure the composition, structure, and circulation of Titan's atmosphere, and characterize the atmosphere-surface interaction;
- Titan's surface: Carry out a reconnaissance of the physical properties and geographic variability of Titan's surface;
- Saturn's atmosphere: Determine the elemental composition, dynamics, and cloud composition and structure, to a level well below the H_2O cloud base;
- Saturn's rings: Measure particle composition and spatial distribution, determine the evolution of dynamic structures, and search for shepherding satellites;
- Saturn's small satellites: Make comparative determinations of surface composition, density, geologic history, and geomorphological processes;
- Saturn's magnetosphere: Specify the structure, dynamics, and processes, and the interactions of the magnetosphere with Saturn's atmosphere, rings, icy satellites, Titan, and the solar wind.

These objectives can be met with an appropriately chosen mission profile and complement of scientific instruments, mounted on a spacecraft with sufficient power and communications capabilities. COMPLEX is favorably impressed with the progress made by the Cassini Project in the design of such a spacecraft and mission. The spacecraft accommodates the Cassini science requirements, while being flexible to mission changes. It appears to be a robust and capable carrier for the Cassini investigations. The level of maturity in the design is high for the current phase of development. It is clear that a number of difficult problems have been solved while maintaining prudent engineering margins. (In addition to adequately meeting the Cassini requirements, the spacecraft will also serve the needs of CRAF.)

The instrument payload selected for Cassini is highly responsive to most of the important science objectives for the Saturn system. Following is a summary of the information that the currently configured mission will be able to obtain for each of the major components of the system.

Titan

Many of the scientific goals for Titan will be addressed by the Huygens probe, the component of the Cassini mission supplied by the European Space Agency (ESA). Its current suite of instruments, complemented by spectroscopic and radar observations from the Cassini orbiter, will do an excellent job of fulfilling these goals by providing a first characterization of Titan's atmosphere and surface. The probe's instruments include gas and haze-particle analyzers, capable imaging and spectral radiometers, as well as atmosphere profilers to determine temperature and pressure. Doppler tracking of the descent will provide the first direct measurement of Titan's atmospheric circulation. Near the surface the probe instruments will measure the composition of the atmosphere, the shock of landing—different for a solid as opposed to a liquid surface—and the density and refractive index of the liquid surface, if present.

Although not in orbit around Titan, the Cassini orbiter will repeatedly pass over Titan's surface and will directly measure the composition of the upper atmosphere. The orbiter's infrared spectrometer will determine temperature and composition globally and as functions of time, complementing the measurements made during the probe's descent. Orbiter imaging at visible and infrared wavelengths will determine haze structure and variability. Properties of the upper atmosphere will be measured during Titan flybys by the orbiter's ion and neutral mass spectrometer. The complementarity of obtaining orbiter data coincident with Huygens probe data is an important advantage of the Cassini mission as currently configured.

As the Cassini orbiter repeatedly passes over Titan, its radar will yield further information on the nature of the surface in high-resolution strip-scans. This will allow imaging of a significant fraction of the surface at a resolution of 1 km or better. The radar will provide information on the composition of the surface and the depth of hydrocarbon oceans or lakes, if they are present. The radar will also operate in a radiometer mode and map surface dielectric constant variations over the entire surface.

In sum, Cassini will represent a major step in achieving COMPLEX's objectives for Titan.

Saturn's Atmosphere

The Cassini orbiter will determine properties of Saturn's atmosphere at all latitudes and will monitor dynamical changes. Infrared spectra will yield composition and temperature throughout the stratosphere and upper troposphere and, when combined with near-infrared and imaging measurements of reflected sunlight, will determine the thermal energy balance both locally and globally. Cloud structure and horizontal atmospheric motions within the upper troposphere will be obtained from temporal imaging sequences. Temperature, pressure, and ammonia abundance will be determined with excellent vertical resolution to a depth corresponding to a pressure of approximately 1 bar by radio occultations. When combined with infrared spectra, radio occultations will also provide an improved determination of the helium abundance.

The Cassini instruments, together with an orbital tour that includes high-latitude coverage, will address all the Saturn atmospheric objectives outlined by COMPLEX except those for inert gas composition and isotopic abundances.

Rings

Throughout the Cassini mission, the orbiter will take images of Saturn's rings at a full range of viewing angles in both reflected sunlight and the thermal infrared. These images will form the database for tracking dynamical effects, wave motions, and spoke kinematics. The orientation of the rings as seen from Earth is particularly favorable for the proposed orbital tour. According to current mission plans, the rings will occult the orbiter 25 times, providing excellent measurements of the rings' transmission and scattering properties at three distinct radio wavelengths. The radio science experiment and complementary ultraviolet stellar occultation data will determine the particle size and mass distributions in all of Saturn's rings as a function of their distance from the planet.

Cassini's complement of selected instruments is sufficient to achieve all the objectives for Saturn ring science set down by COMPLEX.

Saturn's Small Satellites

Besides Titan, Saturn's satellite system includes several small icy bodies, each displaying a variety of surface landforms and evolutionary histories. They have been affected by internal activity, possibly including tectonism and ice volcanism, and external processes, such as impact. The morphology and stratigraphy of the satellites will be evident from the data provided by the imaging system. These data will advance the understanding of satellite surface processes and history, in addition to addressing the thermal history and state of satellite interiors. The spectroscopic instruments will determine the chemical and mineralogical composition of satellite surfaces. The distribution of various compositional units defined by both spectroscopy and imaging will permit the three-dimensional reconstruction of the configuration of the outer crusts of the icy satellites. Cassini's current instrument payload and mission configuration will provide an unprecedented view of the composition, state, and geological evolution of the small icy satellites of Saturn.

The proposed Cassini mission configuration is fully responsive to COMPLEX's goals for the exploration of Saturn's small satellites.

Magnetosphere

The particles-and-fields instruments will be able to measure particle fluxes with good coverage and good resolution of energy, spatial orientation, mass, and time. Measurements of plasma waves will enable the determination of the sources and sinks of magnetospheric plasma. The particles-and-fields instruments also have an excellent capability to characterize the interaction between Titan and Saturn's magnetosphere. Cassini's magnetometer will be able to determine the configuration of Saturn's nearly axially symmetric magnetic field. This, when combined with the directional capabilities of the radio receiver, will allow determination of the origin of the kilometric radio emission modulation and, hence, the characterization of the nonsymmetric components of Saturn's magnetic field. The temporal and spatial (both radial and latitudinal) coverage of Saturn's magnetosphere during the Cassini mission should clearly establish the nature and origin of temporal variations in the magnetosphere.

The Cassini payload and mission design appear to be fully capable of achieving the major scientific objectives of studying Saturn's magnetosphere.

COMPLEX's overall conclusion is that the Cassini mission, as currently configured, is extremely responsive to the highest-order priority for exploring the outer planets, i.e., intensive study of Saturn as a system.

CRAF

The primary objective of CRAF since its inception has been a comet rendezvous. Comets represent some of the least-altered material left from the formation of the solar system. Thus the study of comets yields important constraints on conditions in the early solar nebula. However, ground-based observations of comets are limited by interference from Earth's atmosphere and by the generally poor viewing geometry for comets when they are near the Sun. Furthermore, the presence of a cometary coma makes viewing the nucleus difficult. The first close observations of a comet were obtained in 1985 when the International Cometary Explorer encountered Comet Giacobini-Zinner. Later, in 1986, spacecraft from Europe, Japan, and the Soviet Union completed fast flybys of Comet Halley. COMPLEX concluded in a letter report[3] that these encounters left COMPLEX's objectives for the exploration of comets largely unchanged. These objectives, given in COMPLEX's 1980 report, *Strategy for the Exploration of Primitive Solar System Bodies—Asteroids, Comets, and Meteoroids: 1980-1990,* are as follows (in order of priority):

1. To determine the composition and physical state of the nucleus (determination of the composition of both dust and gas is an important element of this objective);
2. To determine the processes that govern the composition and distribution of neutral and ionized species in the cometary atmosphere; and
3. To investigate the interaction between the solar wind and cometary atmosphere.

COMPLEX has reviewed CRAF four times, and each time concluded[4] that the mission as configured at the time of the review was responsive to the above objectives. The present review considers whether the changes in the mission since the last review, in July 1990, invalidate earlier conclusions. The significant changes were the descoping of the instrument payload, in the fall of 1990, and the change in the mission profile, in the fall of 1991, which delays the date of launch.

The descoping of the instrument payload in the fall of 1990 was forced by a new NASA assessment of the costs and risks involved with the development of the CRAF penetrator experiment (PENL). This assessment led to a programmatic decision to remove PENL and the Scanning Electron Microscope and Particle Analyzer (SEMPA) from the instrument payload. In its July 1990 review, COMPLEX had identified SEMPA as not being as effective as the Comet Ice and Dust Experiment (CIDEX) and the Cometary Matter Analyzer (COMA) in addressing the most important science objective of CRAF, namely, determining the composition and physical state of the nucleus. Thus at the time, COMPLEX stated[5] that SEMPA had lower priority than PENL and that, in spite of the loss of SEMPA, CRAF would remain responsive to COMPLEX's previously stated science goals.

The loss of the penetrator experiment is much more serious. As stated in the same letter, "deletion of the penetrator would severely compromise the ability of the CRAF mission to address the highest-priority goals identified by COMPLEX." PENL was the only experiment that would have sampled the comet nucleus in situ.

As important as the loss of PENL was, however, COMPLEX continues to hold the view, stated in its 1980 report *Strategy for the Exploration of Primitive Solar System Bodies—Asteroids, Comets, and Meteoroids: 1980-1990,* that

[3] Space Science Board letter to Geoffrey A. Briggs, May 27, 1987.

[4] Space Science Board/Space Studies Board letter to Dr. Geoffrey A. Briggs, May 31, 1985; letter to Dr. Geoffrey A. Briggs, May 27, 1987; letter to Dr. Geoffrey A. Briggs, September 1, 1988; letter to Dr. Lennard A. Fisk, August 10, 1990.

[5] Space Studies Board letter to Dr. Lennard A. Fisk, August 10, 1990.

comet "science objectives can be met during the next decade without undertaking to land on or penetrate a comet nucleus." This view is based on the fact that the descoped CRAF will still be able to contribute many things to comet science in response to COMPLEX's primary, near-term objectives. Following is a list of what CRAF would still be able to do:

- Measure the shape and size of the nucleus;
- Measure the mass to better than one percent;
- Accurately compute the bulk density from the mass and volume;
- Measure the mass distribution of the nucleus;
- Map the active and dormant regions of the nucleus to determine why they are different;
- Study the morphology and evolution of craters;
- Characterize the surface energy budget;
- Determine the surface composition;
- Determine the dust composition;
- Determine the gas composition—both neutral and ion species;
- Gain information about the onset of activity in the comet and the formation of the coma;
- Characterize jet features and the relation between dust and gas in jets;
- Study the magnetic field in the coma; and
- Study the tail, including the interaction with the solar wind.

Many of these anticipated results directly address the primary objective of characterizing the nucleus. COMPLEX therefore believes that the descoped CRAF mission is still responsive to its highest-priority near-term goals for comet science. At the same time, COMPLEX reiterates its comments in *Assessment of Solar System Exploration Programs: 1991*[6]: "The 1990 deselection of the CRAF penetrator requires continuing attention to alternative means of directly sampling a comet nucleus. The completion of this goal may still require acquiring and returning a sample of a cometary nucleus in some future mission."

The change in the CRAF mission profile was forced by the congressional budget decisions for FY 92. The change caused the CRAF launch to be delayed from February 1996 to April 1997. This slippage delays the comet rendezvous from 2003 to 2006, but accommodated a cut in the proposed funding for the CRAF/Cassini program in FY 92. This change does not per se cause any loss in the mission's anticipated return for comet science.

The launch slippage does, however, enhance significantly the results anticipated for asteroid science. The primary goals of asteroid exploration, set forth in COMPLEX's 1980 primitive bodies strategy, are to determine the composition, bulk density, and surface morphology of asteroids. Elucidation of the diversity of asteroids is an essential aspect of these goals. The earlier mission profile would have included flybys of only one or two small asteroids, whereas the new profile includes flybys of two large asteroids, 88 Thisbe and 19 Fortuna, and at least one small asteroid, 1084 Tamariwa. The large asteroids are particularly important since they are likely to be primitive, undifferentiated bodies that would provide information about processes of planet formation in the early solar system.

In addition to the three asteroids, the baseline mission now includes a gravity assist from Mars. This will afford an important opportunity to augment knowledge about Mars, using excellent ultraviolet, visible, and near-infrared imaging capabilities not included on currently planned Mars missions. Along with the opportunity to study Mars itself comes the chance to study Phobos and Deimos, its two satellites. CRAF will be able to compare and contrast these irregular satellites with the three asteroid targets.

The slippage of the launch by one year and the comet rendezvous by three years does not come without drawbacks. In addition to the added run-out costs, there is additional risk of component failure. However, the lifetime of the reconfigured mission still falls within the design criterion originally specified for CRAF. Although reduction of power from the radioisotope thermoelectric generators is a concern, the rendezvous nature of this mission, with the spacecraft in prolonged close proximity to the comet, relaxes some of the constraints on power usage and makes power usage less a concern for CRAF than for a more rapid flyby mission. Therefore, COMPLEX concludes that the complications arising from the one-year launch delay for CRAF are more than offset by the enhanced opportunities resulting from NASA's redesign of the mission profile.

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[6] *Assessment of Solar System Exploration Programs: 1991*, Committee on Planetary and Lunar Exploration, Space Studies Board, National Academy Press, Washington, D.C., 1991.

4.5 On the Advanced X-ray Astrophysics Facility

The Space Studies Board sent the following letter to Dr. Lennard A. Fisk, NASA Associate Administrator for Space Science and Applications, on March 30, 1992.

It has reached the attention of the Space Studies Board that, as a response to the severe constraints in the FY 93 budget, the Advanced X-ray Astrophysics Facility (AXAF) mission is currently undergoing a reevaluation to reduce its immediate funding requirements. In the present environment, the mission may be exposed to serious damage or outright cancellation. The Board would like to draw your attention to the extremely high priority that a series of National Research Council (NRC) astronomy advisory committees have attached to this mission over a long period of time. The following quotations from the reports of these committees express the great importance of an advanced x-ray telescope to the U.S. space research program:

- *A Strategy for Space Astronomy and Astrophysics for the 1980s,* report of the Committee on Space Astronomy and Astrophysics of the Space Science Board, National Academy of Sciences, Washington, D.C. (1979):

  > A clear priority in x-ray astronomy is the development of a national facility telescope . . . . The great improvement in sensitivity, the ability to address problems of fundamental importance, the potential for new discovery, and the availability of observing time to a broad range of users combine to make this mission a key feature of the nation's astronomy program, not just that of x-ray astronomers. (page 87)

- *Astronomy and Astrophysics for the 1980s,* Volume 1, report of the NRC Astronomy Survey Committee (the Field Report), National Academy Press, Washington, D.C. (1982):

  > The Committee believes that four major programs are critically important for the rapid and effective progress of astronomical research in the 1980s and is unanimous in recommending the following order of priority:
  >
  > 1. An Advanced X-Ray Astrophysics Facility (AXAF) operated as a permanent national observatory in space, to provide x-ray pictures of the Universe comparable in depth and detail with those of the most advanced optical and radio telescopes. (page 15)

- *Space Science in the 21st Century—Astronomy and Astrophysics,* report of the Task Group on Astronomy and Astrophysics of the Space Science Board, National Academy Press, Washington, D.C. (1988):

  > X-ray astronomy will achieve a substantial increase in observational capability with the anticipated launch of the Advanced X-ray Astrophysics Facility (AXAF) in about 1994. (page 25)
  >
  > The powerful capabilities of AXAF and the wealth of fundamental problems it can address suggest that this facility will advance research for a long time to come. (page 27)

- *The Decade of Discovery in Astronomy and Astrophysics,* report of the Astronomy and Astrophysics Survey Committee of the Board on Physics and Astronomy, National Academy Press, Washington, D.C. (1991):

  > The committee reaffirms the Field Committee decision that made AXAF the highest-priority large program of the 1980s and stresses the importance to all astronomy of deploying AXAF as soon as possible. (page 65)

The NRC is currently transitioning its advisory responsibilities in space astronomical research, previously vested in the Committee on Space Astronomy and Astrophysics and more recently in the Astronomy and Astrophysics Survey Committee, to a new organizational structure. This new committee structure should be available soon to conduct a comprehensive assessment of any AXAF program modifications that might be proposed. In the meantime, the published corpus of NRC advice on AXAF unambiguously asserts the vital importance of this program for scientific advances in the astronomical disciplines.

I would be happy to meet with you to discuss these issues further if it would be helpful.

Signed by
Louis J. Lanzerotti
Chair, Space Studies Board

4.6 On NOAA Requirements for Polar-Orbiting Environmental Satellites

The Committee on Earth Studies sent the following letter to Mr. Russell Koffler, NOAA Deputy Assistant Administrator/NESDIS, on April 30, 1992.

At its recent meeting in Washington, D.C., the Committee on Earth Studies (CES) conducted the preliminary review you requested of the draft document entitled *NOAA Requirements for Support from Polar-Orbiting Satellites.* Although the committee notes omissions and makes suggestions for further work, it finds the draft to be a proper first step in defining an objective and quantitative basis for projecting future observational requirements for polar-orbiting satellites.

The document states many of the measurements that the CES perceives to be required. Because the scope of NOAA's programs is so broad, the cumulative requirements presented correspond, in some respects, to the nation's overall needs for polar-orbiting remote sensing of the atmosphere, oceans, coastal areas, ice, and marine life. Since NOAA is the federal agency charged with meeting these long-term, operational needs, the committee suggests that NOAA consciously survey their requirements from an integrated perspective.

In spite of the document's appearance of inclusiveness, there are shortcomings; for example, the needs of the National Marine Fisheries Service are described in less detail than those of any other NOAA line office. This treatment does not adequately reflect the future needs of the Fisheries Service.

Even nearer term, the document shows no current or projected use of Landsat or Landsat-like data, not even for estuaries and coastal regions. The measurement of sediment plumes from estuaries, pollution, and coastal mapping are all illustrations of important applications of Landsat data for NOAA offices.

The committee recommends that the draft be revised to reflect a change in philosophical perspective. A document that looks decades or more into the future should be more expansive. Indeed, the present document assumes that none of the numerous polar-orbiting research satellites scheduled for launch in the 1990s and early 2000s will affect the instrument complement of NOAA's polar-orbiting operational satellites of the future.

The spacefaring nations of the world are developing new techniques for Earth remote sensing and are advancing past successes. These new capabilities are correctly noted in the draft, but the assumed NOAA baseline system for 2002 and beyond is simply a continuation, admittedly with some modest change, of the observing system that has been in place since the late 1970s. Further, spacefaring and non-spacefaring nations alike are expanding their use of satellite-derived remote-sensing data and improving the application of these data to research and operational needs. With profound changes occurring in technological capability and in the application of enhanced capabilities, it does not appear reasonable for NOAA's baseline to be unaffected over a period of 25 years or more.

The CES recognizes that the nation has severe budget problems today, and may have them again in the future. A multi-decadal planning document, however, should reflect what are evident future possibilities, and not be limited in its vision by current, transitory restrictions. Future budget decisions may indeed restrict NOAA's ability to exploit the latest scientific and technical developments, but the anticipation that adverse budget decisions will be made at some hypothetical point in the future should not constrict the consideration of future prospects.

In planning the observing systems to be deployed 10 or more years in the future, NOAA should not only build upon what is known at present, but also make plausible projections of what will be learned from systems to be flown in the interim. NOAA should also examine the advances in the use of data from these and operational systems. Planning must assume an intellectually plausible degree of success and then be flexible enough to respond to greater or lesser degrees of success. Thus NOAA must project both its line-offices' needs and the results of missions that are currently under way and/or approved for the next decade. This is not done in the section of the draft that relates the planned satellite baseline to future requirements.

An example regarding ocean measurements should clarify this point. NOAA has the charter to provide weather, marine, and climate forecasts. There is no rationale that would suggest that society's needs for such forecasts will diminish in the future. However much such forecasts improve, society will continue to demand even more improvement.

To be more specific, our need to understand the ocean surface better is an evident and increasingly important objective for many user communities. Regular marine operations, whether for fishing or the transport of goods, require a better knowledge of present conditions and an improved prediction of future conditions. Likewise, the production of improved weather forecasts through numerical computer models will require a better characterization of the atmosphere-ocean interface (fluxes of heat, moisture, gases, and momentum on synoptic time scales). Still further, the understanding of climate change requires an improved understanding of ocean circulation and basin-scale air-sea interaction. Thus three communities of users quite predictably will seek a better knowledge of ocean conditions.

There are a number of instruments that can contribute to a better understanding of the ocean, but two—the radar altimeter and scatterometer—have become a consensus choice, if consensus is measured by the number of such instruments that have been flown or will be flown in space by a number of nations. The state of development of these instruments has progressed to the extent that the relevant communities are convinced that their long-term and operational use will contribute to the objectives of improved support to marine operations, weather forecasting, climate prediction, and marine research in general. The CES notes that all of these applications require a continuing, permanent observational capability and that the instruments must be designed to produce scientifically useful data of sufficient accuracy, resolution, and spatial coverage. The instruments will evolve and data processing techniques will continue to improve, but the measurement of sea conditions must continue permanently.

In this instance, the assumption of success is less speculative than in almost any multi-decadal projection the CES members can imagine. Therefore, when planning observational systems for operation beyond 2002, NOAA's baseline should either include altimeters and scatterometers on its satellites or secure data of comparable quality on an operational basis from other national or international partners. NOAA has made a commendable effort to use data collected by other organizations, and should continue to do so in the future. However, if the data are indeed *required,* NOAA must ensure that the agreements it enters into address explicitly not only quality and type, but also the continuity, reliability, and freedom of use necessary to meet the needs of NOAA's operational and research constituencies.

In sum, the CES believes that the draft report *NOAA Requirements for Support from Polar-Orbiting Satellites* is a necessary start. The report should, however, be recirculated within NOAA to ensure its completeness. This is particularly needed with respect to the treatment of National Marine Fisheries Service requirements and of the potential use of Landsat or Landsat-like data in all of the line offices. Most importantly, however, the planning perspective for the early 2000s should be considerably more aggressive for all of NOAA's offices.

The committee believes that the document should be revised and completed promptly, and then periodically reviewed and updated as programs evolve. In particular, the document's statement of physical parameters to be measured by satellites, together with their spatial, spectral, and temporal resolution and their corresponding precision, needs to be evaluated on a continuing basis.

The CES will continue its assessment of the revised NOAA draft document at future meetings. The committee also intends to study the requirements of some of NOAA's line offices in more detail. To carry out this review, it will be necessary for CES to obtain a better understanding of the overall flow of polar-orbiting satellite data and the related ground processing systems. One topic that the committee especially wishes to investigate is how improvements in numerical weather and climate forecasting models will drive the requirements for data from operational satellites.

Signed by
John H. McElroy
Chair, Committee on Earth Studies

4.7 On Continued Operation of the BEVALAC Facility

The Space Studies Board and its Committee on Space Biology and Medicine addressed the following letter to Secretary of Energy James D. Watkins and NASA Administrator Daniel J. Goldin on August 20, 1992.

On May 14, 1992, the Committee on Space Biology and Medicine (CSBM) of the Space Studies Board (SSB) was briefed by the acting director of NASA's Life Sciences Division, Mr. Joseph K. Alexander, concerning various issues and activities in which the division is engaged. Among the issues raised was the impending decommissioning of the BEVALAC at the Lawrence Berkeley Laboratory as outlined in correspondence from Dr. David Hendrie, director of the Department of Energy's Division of Nuclear Physics. Subsequently, the CSBM discussed this issue with the Board at its meeting in Huntsville, Alabama, in June.

The Board and the CSBM are in agreement with a host of advisory committees' recommendations concerning the importance of gaining a better understanding of the biological effects of high Z element (HZE high-energy) particles.[1] Critical to planning for extended human sojourns in deep space is quantitative knowledge about the dose rates and types of radiation that will be encountered and the related biological effects.

The SSB and CSBM are concerned about the closing of the BEVALAC given that there is no alternative facility at which to continue the radiobiological research conducted as part of this country's goal of expanding the human presence in space. This facility is the only accelerator in the United States capable of producing the spectrum of energies required for research concerning the physical and biological effects of the heavy ions that will be encountered during deep-space missions. Providing adequate shielding against radiation and taking other measures to protect astronauts during deep-space travel are directly dependent on information derived from research concerning the biological effects of protons and HZE particles.

It is our understanding that even if funding for an alternative facility were provided today, there would be at least a five-year hiatus before suitable beams could become available. An interruption of the radiobiological research currently under way at the BEVALAC would have a number of deleterious effects on this well-established program that is a critical component of the national goal of human space exploration. Research teams that have been assembled to conduct this work would disperse and transfer to other areas of research. The flow of valuable long-term data derived from the BEVALAC studies would cease. Thus it would be necessary to start all over with new research animals, when another accelerator became available, in order to obtain data from repeated, increasingly longer periods of exposure—a condition absolutely crucial to this type of research. Finally, losing this capability would seriously damage the research program of the recently established NASA Specialized Center for Research and Training (NSCORT) in Space Radiation Health at Lawrence Berkeley Laboratory and contribute to the loss of expertise in basic radiobiological research—an outcome that would be contrary to the conclusion reached in NASA's Space Radiation Health Program Plan.[2]

> There is an acute need for additional well-trained and well-qualified researchers in space radiation physics and biology. A continuous supply of trained space researchers needs to be developed and adequate numbers of trained personnel need to be available to enable program expansion. (p. 30)

Various heavy-ion facilities exist worldwide that could, theoretically, support the type of space-related research under way at Berkeley. However, the SSB and CSBM have no evidence that any of these facilities could be made available to support NASA's HZE radiation research program. The BNL Booster at Brookhaven National Laboratory has limited capability, and no beam time will be available until a new irradiation facility is built. The Darmstadt accelerator has provisions for cell research but not for animal research, and beam time at the facility is currently oversubscribed by a factor of two. The JINR at Dubna has obsolete equipment, low beam intensity, and beam contamination—significant limiting factors. The Synchrophasotron at Saclay has no provisions for conducting animal or cell research, and at least a year would be required to prepare the facility to provide iron beams. Beams generated at the facility at Geneva are beyond the energy range required by NASA researchers. Finally, the accelerator at Chiba is not yet in operation and will not produce iron ion particles.

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[1] Attachments citing 14 supporting statements drawn from internal NASA and advisory documents and NRC reports accompanied the original correspondence; they are here appended to the letter.

[2] Space Radiation Health Program Plan, Life Support Branch, Life Sciences Division, NASA, Washington, D.C., November 1991.

Understanding that the NASA-sponsored research at the BEVALAC may be relatively minor in the context of the Department of Energy's (DOE) overall mission, the SSB and CSBM believe that the decision to decommission this facility should be considered in the context of the importance of the BEVALAC to the U.S. space program—one in which DOE plays an increasing role.[3] Until a suitable alternative can be provided that supports research related to long-term plans for human space exploration, the SSB and CSBM urge that the BEVALAC remain available to NASA researchers. Given the importance of the radiobiological research conducted at the BEVALAC and its fundamental role in realizing the national goal of human space exploration, the SSB and CSBM strongly recommend that DOE and NASA agree on a means for continuing without interruption the capability now provided by the BEVALAC.

Signed by
Louis J. Lanzerotti
Chair, Space Studies Board
and
Fred W. Turek
Chair, Committee on Space Biology and Medicine

EXCERPTS AND RECOMMENDATIONS CONCERNING BIOLOGICAL EFFECTS OF RADIATION EXPOSURE

It is critical for NASA to formalize agreements to utilize one or more of the federal accelerator facilities, and to assure that those facilities remain in operation until necessary ground-based research is completed.

—Aerospace Medicine Advisory Committee/NASA Advisory Council, *Strategic Considerations for Support of Humans in Space and Moon-Mars Exploration Missions, Life Sciences Research and Technology Programs.* 1992

In order to protect crews, to the extent possible, from the various harmful effects of radiation, it is necessary to thoroughly characterize the radiation environment, understand the biological effects of HZE radiation and protons (leading to the establishment of appropriate risk levels and limits for radiation exposure), and accurately predict and provide warning of any increased levels of radiation.

—Discipline Working Group on Radiation Health and Environmental Health, NASA, *Space Radiation Health Program Plan.* 1991

Determining the long-term medical consequences of exposure to high Z element (HZE) particles present as a component of galactic cosmic radiation (GCR) is critical. The biological hazards associated with HZE particles, i.e., the "late effects," are not adequately known and may pose unacceptable long-term cancer risks. Exposure can result in life-threatening and life-shortening effects, such as cancer, and other detrimental consequences including cataract formation, mutagenesis, and other tissue damage.

—Aerospace Medicine Advisory Committee/NASA Advisory Council, *Strategic Considerations for Support of Humans in Space and Moon-Mars Exploration Missions, Life Sciences Research and Technology Programs.* 1992

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[3] National Space Policy Directive for Space Exploration Initiative Strategy, Section III, paragraphs *c* and *d*, March 13, 1992.

NASA should make a commitment to support fundamental research on the biological effects of radiation. This support and commitment should take the form of expanding NASA's role in and funding for basic research and of contributing to the necessary facilities, such as the BEVALAC accelerator.

—Life Sciences Strategic Planning Committee, NASA, *Exploring the Living Universe—A Strategy for Space Life Sciences.* 1988

In summary, the highest priorities are for improved dosimetry and for studies of the effects of HZE particles so that the risks of both stochastic effects, such as carcinogenesis, and nonstochastic effects such as CNS damage, can be estimated with confidence.

—National Council on Radiation Protection and Measurements, *Guidance on Radiation Received in Space Activities.* 1989

One concern requiring further study in this area is the high-energy high-charge component of the cosmic ray flux, which can damage non-dividing cells, including those of the central nervous system.

—National Commission on Space, *Pioneering the Space Frontier.* 1986

The Space Exploration Initiative requires understanding and management of space radiation hazards. Uncertainties in these radiation effects on cells, tissue and small organisms could be reduced by simulations using the Bevalac at the Berkeley Radiation Laboratory.

—Synthesis Group, NASA, *America at the Threshold.* 1991

REPORTS FROM THE NATIONAL RESEARCH COUNCIL

The availability of HZE particles for experimental radiation biology is extremely limited. The only feasible approach to obtaining the required information is to carry out controlled studies in adequate ground-based facilities.

—Radiobiological Advisory Panel/Space Science Board, *Radiobiological Factors in Manned Space Flight.* 1967

The availability of a ground-based accelerator capable of producing HZE particles now permits the design of precisely ordered experiments. Such experiments should be supported.

—Committee on Space Biology and Medicine/Space Science Board, *A Strategy for Space Biology and Medical Science for the 1980's and 1990's.* 1987

It is important to learn more about the relative biological effects of radiation influences, particularly high-Z galactic cosmic rays and solar flare electrons and their relationship to cancer and cataract induction in order to set meaningful guidelines for radiation protection. The question of appropriate shielding in flight is complex and requires further study.

—Committee on Human Exploration of Space/National Research Council, *Human Exploration of Space—A Review of NASA's 90-Day Study and Alternatives.* 1990

Terrestrial studies of the biological effects of low-level, high LET irradiation on cell cultures and animals (using particle accelerators) should be expanded, with particular attention paid to the space radiation problem.

—Life Sciences Task Group, Space Studies Board, *Space Science in the Twenty-First Century—Imperatives for the Decades 1995 to 2015—Overview and Life Sciences.* 1988

Planning for extended human sojourns in space mandates that we have quantitative knowledge about the dose rates and the types of radiation that will be encountered. Similarly, the effects of the different types of radiation encountered in space, especially deep space, must be defined quantitatively. Much of the necessary radiobiology research can be carried out on Earth with defined radiation sources.

—Committee on Space Biology and Medicine/Space Studies Board, *Assessment of Programs in Space Biology and Medicine—1991*. 1991

One way to maximize the return on investment in research is through various modes of cooperative research, with foreign partners, private concerns, and between federal agencies. . . . [An] example for . . . collaboration between federal agencies are facilities supported by the Department of Energy such as the BEVALAC, which has the capability of providing for study of very high-Z particles and their biological effects.

—Space Studies Board, *Priorities in Space Life Sciences Research,* testimony by Space Studies Board Member Robert H. Moser to the House Budget Committee Task Force on Defense, Foreign Policy and Space, April 28, 1992

Improved measurements of cross-sections and better modeling of heavy-ion interactions, particularly for the yield and spectra of neutrons and other secondary particles generated in the shielding material, are also required. NASA currently helps support the BEVALAC heavy-ion accelerator and some cross-section studies. However, the BEVALAC has been threatened with closure, thus endangering some of the enabling research on both cross-section measurements and the long-term biological effects of ionizing radiation.

—Committee on Human Exploration/Space Studies Board, *Scientific Prerequisites for the Human Exploration of Space*. 1993, in press

4.8 On Robotic Lunar Precursor Missions of the Office of Exploration

The Space Studies Board and its Committee on Planetary and Lunar Exploration addressed the following letter and attached assessment to Dr. Michael D. Griffin, NASA Associate Administrator for Exploration, on August 21, 1992.

As you know, the Space Studies Board is the National Research Council's primary advisory body for civil space research.[1] Within this broad scope, the charter of the Board's Committee on Planetary and Lunar Exploration (COMPLEX) is to advise the Board on "the entire range of planetary system studies that can be conducted from space."[2] This advisory purview extends across the entire spectrum of U.S. space research conducted by both "NASA and other government agencies."[3] COMPLEX's advisory capacity thus includes planetary science aspects of the Space Exploration Initiative (SEI) being implemented by NASA's Office of Exploration (OEXP).

At COMPLEX's April 27-28, 1992, meeting in Washington, D.C., you briefed committee members on the report, *Workshop on Early Robotic Missions to the Moon*,[4] sponsored by your office, and on the current status of OEXP's lunar exploration program. The attached scientific assessment, prepared by COMPLEX based on information provided during this meeting, gauges the extent to which the flight program outlined in the LPI report addresses the recommendations that COMPLEX has made on priorities for lunar science.

In your presentation to COMPLEX, you noted that the federal budget development schedule obliged OEXP to initiate a rapid assessment of the possible instruments for its proposed lunar orbiters and lander, precluding a more widely publicized and more formally peer-reviewed instrument evaluation. The Board notes that a broadly based selection process that includes peer review has served the science community very well in the past; it has helped NASA accomplish its goals, while assuring fair competition and the best possible science. As a result, although the Board and committee appreciate your efforts to act expeditiously and streamline the procurement process, it is recommended that future OEXP review and selection processes promote and actively facilitate the widest possible community participation.

Overall, COMPLEX's assessment suggests that the program of robotic lunar exploration that the committee reviewed presents a significant opportunity to advance scientific investigation of the Moon. It is emphasized that the committee's assessment is limited to these lunar science objectives and does not consider the separate issue of the adequacy of data from the proposed flight program to support a subsequent program of human exploration. The Board and COMPLEX look forward to providing further guidance on scientific aspects of the SEI on a continuing basis.

Signed by
Louis J. Lanzerotti
Chair, Space Studies Board
and
Larry W. Esposito
Chair, Committee on Planetary and Lunar Exploration

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[1] Letter of National Academy of Sciences President Detlev Bronk to Space Science Board Chair Lloyd Berkner, June 26, 1958.

[2] *Assessment of Solar System Exploration Programs: 1991*, Committee on Planetary and Lunar Exploration, Space Studies Board, National Academy Press, Washington, D.C., 1991, page 3.

[3] Reference 2, page 3.

[4] *Workshop on Early Robotic Missions to the Moon*, Lunar and Planetary Institute, Houston, Texas, February 1992; proceedings of a workshop sponsored by NASA's Office of Exploration.

SCIENTIFIC ASSESSMENT OF PROPOSED ROBOTIC LUNAR MISSIONS OF NASA'S OFFICE OF EXPLORATION

The Space Studies Board's Committee on Planetary and Lunar Exploration (COMPLEX) met in Washington, D.C., on April 27-28, 1992, to review the robotic lunar probes proposed by NASA's Office of Exploration (OEXP) as precursor missions for its program of human exploration. The committee's intention was to determine the extent to which OEXP's program was consistent with recommendations for lunar science outlined in previous COMPLEX reports.[1,2,3]

During its deliberations, COMPLEX was briefed on the current status of OEXP's lunar exploration program by Dr. Michael D. Griffin, NASA's Associate Administrator for Exploration. The committee was particularly keen to discuss the Lunar and Planetary Institute (LPI) report, *Workshop on Early Robotic Missions to the Moon*.[4] This report, sponsored by OEXP, outlines the instrument complement for a flight program of two lunar orbiters and a single lander intended by OEXP to gather scientific and engineering data necessary to facilitate the safe return of humans to the Moon. While COMPLEX recognizes that these are not primarily scientific missions, they have the potential to gather data addressing scientific questions discussed in past COMPLEX reports.

The committee concludes that the LPI workshop's recommendations for the instrument payloads of Orbital Missions 1 and 2 are responsive to the priorities for lunar science stated in past COMPLEX reports. In particular, Orbiter Mission 1 would address COMPLEX's highest-priority goal for lunar exploration, to "determine the chemistry of the lunar surface on both a global and regional scale."[5]

The committee has some concerns, however, about instrumentation proposed to perform these observations. The LPI report states that the instrument of choice is a "gamma-ray/neutron spectrometer with a germanium detector." COMPLEX concurs with this preference but disagrees with the report's subsequent advice that "if development concerns arise regarding this detector, we recommend use of a NaI detector with subsequent flight of a germanium system."[6] Because the spectral resolution of the germanium detector is far superior to that of the NaI detector, COMPLEX recommends that OEXP fly a germanium detector, even at the expense of a modest launch delay.

With regard to the other proposed instruments for Orbital Missions 1 and 2, COMPLEX finds that the proposed visible and infrared imaging spectrometers as well as the imaging, laser altimetry, and gravity mapping (using two spacecraft for far-side gravity determinations) experiments would satisfy high-priority measurement objectives that COMPLEX has presented previously.[7]

The lander mission appears to have scientific value, but its present lack of definition precludes an adequate assessment of its potential contribution to the achievement of COMPLEX's objectives.

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[1] *Strategy for Exploration of the Inner Planets: 1977-1987*, Committee on Planetary and Lunar Exploration, Space Science Board, National Academy of Sciences, Washington, D.C., 1978.

[2] *1990 Update to Strategy for Exploration of the Inner Planets*, Committee on Planetary and Lunar Exploration, Space Studies Board, National Academy Press, Washington, D.C., 1990.

[3] *Assessment of Solar System Exploration Programs: 1991*, Committee on Planetary and Lunar Exploration, Space Studies Board, National Academy Press, Washington, D.C., 1991.

[4] *Workshop on Early Robotic Missions to the Moon*, Lunar and Planetary Institute, Houston, Texas, February 1992; proceedings of a workshop sponsored by NASA's Office of Exploration.

[5] Reference 3, page 7.

[6] Reference 4, page 2.

[7] Reference 2, page 18.

4.9 On the NASA/SDIO Clementine Moon/Asteroid Mission

The Space Studies Board and its Committee on Planetary and Lunar Exploration sent the following letter and assessment to Dr. Simon P. Worden, Deputy for Technology of the Strategic Defense Initiative Organization, and Dr. Wesley Huntress, Director of the Solar System Exploration Division at NASA, on August 21, 1992.

The Integrated Sensor Experiment (hereafter referred to as the Clementine mission) is primarily a Department of Defense mission intended to perform space verification of certain spacecraft subsystems. In one mission concept for the experiment, scientific data about the Moon and an asteroid could be returned while achieving the mission's primary objectives. As a result, the Strategic Defense Initiative Organization (SDIO) has engaged NASA's participation in mission planning in a consultative role regarding potential science content of the mission.

The Space Studies Board is the National Research Council's principal advisory body for civil space research.[1] In support of this broad responsibility, the Committee on Planetary and Lunar Exploration (COMPLEX) is charged with advising the Board on "the entire range of planetary studies that can be conducted from space."[2] This advisory purview includes "carrying out studies, monitoring the implementation of science strategies, and providing recommendations to NASA and other government agencies."[3] Thus, the Board and COMPLEX have examined the proposed mission and its position in NASA's wide-ranging program of planetary exploration.

COMPLEX and the Board recognize that initiation of the Clementine mission and of its possible successors raises a number of national science policy questions beyond the technical issues addressed here. Please note that the Board recommendations presented in this letter and in the accompanying scientific assessment prepared by COMPLEX do not address these broader issues. The letter and assessment also do not consider any aspects of the responsiveness of the Clementine mission to national security needs.

In this context, COMPLEX was briefed on the Clementine mission at a meeting in Washington, D.C., on April 27-28, 1992. The attached assessment presents the committee's analysis of the extent to which the expected data return from Clementine addresses past COMPLEX recommendations for scientific exploration of the Moon and asteroids.[4,5,6] In this assessment, COMPLEX concludes that "Clementine's observations of the Moon and of the asteroid 1620 Geographos provide a significant opportunity to advance our scientific understanding of these objects."

COMPLEX also advises, and the Board concurs, that maximizing the scientific return from Clementine requires that a group of researchers be charged specifically with responsibility for carrying out the scientific aspects of the mission. Some aspects of these responsibilities (principally filter selection—see Attachment) are currently being discharged by a Clementine Science Working Group assembled by NASA's Office of Space Science and Applications (OSSA). In past NASA planetary missions, selection of science teams by peer review has helped ensure fairness and the best achievable science return by engaging the broadest possible community involvement in planning and execution of these missions. The Board and committee therefore recommend that NASA establish a science team for Clementine through a peer review process. Since this recommendation is not intended to reflect adversely on the membership of the current ad hoc Science Working Group, one approach would be to appoint the present Science Working Group members to the science team and then enlarge this science team by a traditional peer review process. The expanded duties of this team would include conducting, or assisting with, scientific calibration and documentation of the selected instruments, validating and archiving scientific data, possibly carrying out scientific studies with the acquired data, and, where appropriate, executing ground-based correlative studies. The science team should remain in place throughout the duration of the mission. The Board notes that it would recommend a peer-reviewed

[1] Letter of National Academy of Sciences President Detlev Bronk to Space Science Board Chair Lloyd Berkner, June 26, 1958.

[2] *Assessment of Solar System Exploration Programs: 1991*, Committee on Planetary and Lunar Exploration, Space Studies Board, National Academy Press, Washington, D.C., 1991, page 3.

[3] Reference 2, page 3.

[4] *1990 Update to Strategy for Exploration of the Inner Planets*, Committee on Planetary and Lunar Exploration, Space Studies Board, National Academy Press, Washington, D.C., 1990.

[5] *Strategy for Exploration of the Inner Planets: 1977-1987*, Committee on Planetary and Lunar Exploration, Space Science Board, National Academy of Sciences, Washington, D.C., 1978.

[6] *Strategy for the Exploration of Primitive Solar-System Bodies—Asteroids, Comets, and Meteoroids: 1980-1990*, Committee on Planetary and Lunar Exploration, Space Science Board, National Academy of Sciences, Washington, D.C., 1980.

approach to science team formation for flight programs in any of the other space research disciplines within its purview.

In connection with the duties of a science team, the Board and committee recommend further that the Clementine mission incorporate plans and procedures for data archiving to provide data access by the scientific community at large. A successful scheme for the management of scientific data should embody the following principles:[7]

- data formats to be designed for ease of use by scientists,
- appropriate ancillary data to be supplied with primary data,
- data to be processed and distributed in a timely manner, and
- proper documentation to accompany all data sets that have been validated and prepared for archival storage.

NASA's Planetary Data System (PDS) was established in accordance with these principles, and the Board and committee recommend that scientific data from Clementine be deposited in the NASA PDS to facilitate use by the scientific community at large.

The Board notes that an effective collaboration between NASA and SDIO on this mission may require that OSSA assume certain responsibilities (such as those recommended above) and cover corresponding expenses in support of the scientific component of the mission. In doing so, OSSA should carefully scrutinize these projected costs and evaluate them in light of the expected science returns and competing budgetary needs of other deserving space research programs.

The Board and COMPLEX look forward with interest to the development of the Clementine mission and plan to offer continued guidance on scientific aspects of the program.

Signed by
Louis J. Lanzerotti
Chair, Space Studies Board
and
Larry W. Esposito
Chair, Committee on Planetary and Lunar Exploration

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[7] *Data Management and Computation, Volume I: Issues and Recommendations*, Committee on Data Management and Computation, Space Science Board, National Academy Press, Washington, D.C., 1982.

SCIENTIFIC ASSESSMENT OF THE STRATEGIC DEFENSE INITIATIVE ORGANIZATION'S INTEGRATED SENSOR EXPERIMENT (CLEMENTINE)

The Space Studies Board's Committee on Planetary and Lunar Exploration (COMPLEX) conducted a review of the Strategic Defense Initiative Organization's (SDIO) Integrated Sensor Experiment (hereafter referred to as the Clementine mission) at its April 27-28, 1992, meeting in Washington, D.C. The committee's intent was to determine the extent to which the possible collateral scientific return from this technology demonstration mission might address past COMPLEX recommendations for scientific exploration of the Moon and asteroids.[1,2,3]

During its meeting, the committee was briefed by Drs. Peter Rustan (SDIO Clementine project manager), Carl Pilcher (NASA scientific liaison to the Clementine mission), and Carle Pieters and Eugene Shoemaker (members of the mission's Science Working Group). Based on these presentations, COMPLEX concludes that Clementine's observations of the Moon and of the asteroid 1620 Geographos provide a significant opportunity to advance our scientific understanding of these objects. Although the planned observations cannot completely satisfy the highest scientific objective—determining the geochemistry of the Moon[4,5]—due to lack of imaging spectroscopy and gamma-ray spectroscopy, they will provide a global map of lunar lithology and will substantially improve our geodetic and topographic knowledge of the Moon. Likewise, Clementine's asteroid observations will contribute to COMPLEX's foremost asteroid science objective, which is to "determine their diversity of composition and structure."[6] This contribution will, however, be incomplete because of the rapidity of the flyby, Clementine's limited instrument complement, and its inability to measure the asteroid's mass.[7]

The filters provisionally selected for Clementine's imaging systems will be capable of distinguishing different surface units and will measure well the range of variability of surface rock types. Close-up observations of another asteroid (in addition to Galileo's observations of 951 Gaspra in November 1991, and planned observations of 243 Ida in August 1993) are, in themselves, scientifically valuable.

COMPLEX concludes that new discoveries are possible from this mission and that technical demonstration of SDIO's innovative lightweight sensors on a small mission may provide valuable experience and new options for future planetary missions developed by NASA. The Clementine mission is, therefore, supportive of previous COMPLEX statements[8,9] concerning NASA's development of a small-mission program for planetary exploration.

The committee was concerned, however, by the apparent absence from Clementine program planning, as presented at the April meeting, of formalized procedures[10] for:

- ensuring the scientific calibration and documentation of the selected instruments,
- validating and archiving scientific data,
- performing scientific studies using the acquired data, and
- conducting appropriate ground-based correlative studies.

COMPLEX recommends that SDIO and NASA act together to strengthen these scientifically vital aspects of the program.

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[1] *1990 Update to Strategy for Exploration of the Inner Planets*, Committee on Planetary and Lunar Exploration, Space Studies Board, National Academy Press, Washington, D.C., 1990.

[2] *Strategy for Exploration of the Inner Planets: 1977-1987*, Committee on Planetary and Lunar Exploration, Space Science Board, National Academy of Sciences, Washington, D.C., 1978.

[3] *Strategy for Exploration of Primitive Solar-System Bodies—Asteroids, Comets, and Meteoroids: 1980-1990*, Committee on Planetary and Lunar Exploration, Space Science Board, National Academy of Sciences, Washington, D.C., 1980.

[4] *Assessment of Solar System Exploration Programs: 1991*, Committee on Planetary and Lunar Exploration, Space Studies Board, National Academy Press, Washington, D.C., 1991, page 7.

[5] Reference 1, page 18.

[6] Reference 4, page 15.

[7] Reference 3, page 50.

[8] Reference 2, page 17.

[9] Reference 4, pages 30 and 31.

[10] *Data Management and Computation, Volume 1: Issues and Recommendations*, Committee on Data Management and Computation, Space Science Board, National Academy Press, Washington, D.C., 1982.

4.10 On the Restructured Cassini Mission

The Space Studies Board and Committee on Planetary and Lunar Exploration sent the following letter and assessment to Dr. Lennard A. Fisk, NASA Associate Administrator for Space Science and Applications, on October 19, 1992.

At its meeting on July 16 and 17, 1992, a panel of the Space Studies Board's Committee on Planetary and Lunar Exploration (COMPLEX), chaired by Professor Reta Beebe of New Mexico State University, carried out a review of the restructured Cassini mission. A previous letter, dated March 30, 1992, conveyed to you the results of an earlier review of the CRAF and the pre-restructured Cassini missions. The current COMPLEX report was subsequently discussed at a meeting of the Space Studies Board on August 29, 1992. This letter and attached assessment by COMPLEX present the views of the Board and COMPLEX on the Cassini mission as it is now defined.

Please note that the information supplied to the Board and COMPLEX was insufficient to allow the Board to evaluate either the realism or accuracy of the budget profile projected for the restructured mission. The Board notes, however, that past experience shows that deferring needed expenditures in order to flatten a program's yearly funding profile can raise the overall cost of a mission.

While recognizing the loss of some science content as a result of the restructuring, COMPLEX has concluded that the new Cassini mission remains responsive to the committee's highest priority for outer planet exploration, the intensive study of the Saturn system. The restoration of the cruise science, however, is strongly urged for both operational and scientific reasons.

Signed by
Louis J. Lanzerotti
Chair, Space Studies Board
and
Joseph A. Burns
Chair, Committee on Planetary and Lunar Exploration

SCIENTIFIC ASSESSMENT OF THE RESTRUCTURED CASSINI MISSION

At its meeting in Irvine, California, on July 16 and 17, 1992, a panel of the Space Studies Board's Committee on Planetary and Lunar Exploration (COMPLEX), chaired by Professor Reta Beebe of New Mexico State University, carried out a review of the reconfigured Cassini mission. The committee had previously issued a series of letter reports on the CRAF and Cassini missions.[1,2,3,4] Since the most recent review in March 1992,[5] the mission has been restructured in order to decrease cost, increase mission resiliency and design margins, and eliminate the dependence on the Titan solid rocket motor upgrade. Because this restructuring affects the science capability of the spacecraft and the instrument complement, COMPLEX has assessed the mission in terms of the scientific impact of the revisions. A detailed examination of the budgetary and management revisions was outside the scope of the committee's advisory role, however, and COMPLEX therefore has chosen to evaluate the restructured mission in terms of its potential contribution to post-Voyager knowledge of the saturnian system. Ronald F. Draper (deputy project manager), Richard L. Stoller (spacecraft system engineer), William M. Fawcett (science instruments manager), and Ellis D. Miner (science manager) of the Jet Propulsion Laboratory (JPL) represented the Cassini project in this review.

The Voyager 1 and 2 flybys of the giant planets, including Saturn, were very successful and returned a wealth of scientific information. Nevertheless, high-resolution data were obtained for only a few days for each of the planetary systems. Accordingly, the Cassini mission has been designed to carry out the second phase of the

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[1] Space Science Board letter to Dr. Geoffrey Briggs, May 31, 1985.
[2] Space Science Board letter to Dr. Geoffrey Briggs, May 27, 1987.
[3] Space Science Board letter to Dr. Geoffrey Briggs, September 1, 1988.
[4] Space Studies Board letter to Dr. Lennard Fisk, August 10, 1990.
[5] Space Studies Board letter to Dr. Lennard Fisk, with attached assessment, "Scientific Assessment of the CRAF and Cassini Missions," March 30, 1992.

exploration of Saturn, that of extending the spatial and temporal resolution of observations in order to better characterize the planet and its physical environment. The mission utilizes a Venus-Venus-Earth-Jupiter trajectory with a launch in late 1997. For two years after launch the spacecraft will orbit within two astronomical units of the Sun, gaining energy using gravitational assists from Venus flybys in April 1998 and June 1999 and then returning back past Earth in August 1999. Cassini will receive an additional boost during a near pass of Jupiter in January 2001 and will continue on to Saturn, arriving in late 2004. The spacecraft will investigate the saturnian system for four years, providing an opportunity to make detailed observations with sophisticated instruments for an extended time.

COMPLEX's previous reviews concerned a baseline Cassini mission, which utilized an orbiting spacecraft equipped with an articulated platform containing instrumentation capable of imaging the planet, rings, and satellites in visible and near-infrared light. A second, spinning platform housed instruments for determining the spatial and temporal variations of the magnetosphere and its charged-particle population. This orbiting spacecraft also carried a radar system for mapping Titan's cloud-shrouded surface and the Huygens probe (supplied by the European Space Agency) for characterizing Titan's atmosphere and surface in situ. This combination of instruments was judged to be highly responsive to the goals for the exploration of the outer solar system as previously defined by COMPLEX.[6]

The major differences between the restructured Cassini mission and the baseline mission are the substitution of body-fixed instruments for the attached platforms, deferment of the development of the ground system and flight software to post-launch, and the cancellation of all cruise science. A modest reduction of instrument capability also occurred.

The following limitations have been imposed on the mission:

- The restructured mission has reduced remote sensing capability. The necessity of pointing the entire spacecraft, rather than just a scan platform, means that observations take longer. In addition, rapidly moving targets, such as the inner moons, are difficult to view. The continually changing spacecraft orientation may also make it more difficult to accurately reconstruct the magnetic field configuration.
- All remote sensing data must be recorded for later transmission because the antenna cannot stay pointed at Earth while the instruments are in use.
- It will be more difficult to infer the full three-dimensional distribution of charged particles because the body-fixed instruments will sample at an angular rate of only 0.25°/second (accomplished by spinning the spacecraft about its major axis), or perhaps 1.0°/second if motors are attached to individual instruments. Originally the turntable rotated at 18°/second.
- Spacecraft stability has been reduced. The currently estimated stability will produce narrow-angle images of quality similar to that of the Saturn images obtained by Voyager 1 and 2. The extent to which this can be achieved will be known by December 1992.

On the other hand, the restructured mission also provides the following desirable characteristics:

- Approximately 20% cost savings are projected.
- No saturnian science objectives are lost. The main impact is the reduction of observing time available to each investigating team. Nevertheless, even though fewer data will be taken per orbit, any set of observations needed to address a particular scientific question can be planned.
- Previously, the power supplied by aging radioisotope thermoelectric generators would have been insufficient for alternate mission scenarios with longer flight times. Power requirements on the restructured mission have been reduced to a level that makes all backup missions viable.
- The reduction in mass allows the mission to be flown with the Titan launcher's standard solid rocket motor and does not require the proposed upgraded version, the solid rocket motor upgrade.

COMPLEX believes that the minimization of impact on the science is a result of the close cooperation between JPL personnel and the science teams, including the foreign partners in the mission.

The instrument capabilities of the Cassini mission are still considerably greater than those of its Voyager predecessors, and its four years in orbit around Saturn will allow unprecedented studies of the planet's magnetosphere, rings, atmosphere, and satellites. Cassini thus responds to a past COMPLEX recommendation[7] that reconnaissance

[6] *A Strategy for Exploration of the Outer Planets: 1986-1996*, Committee on Planetary and Lunar Exploration, Space Science Board, National Academy Press, Washington, D.C., 1986.
[7] Reference 6.

missions, such as Voyager, be followed by missions of intensive study. The Cassini orbital mission, with its extended duration and deployment of the Titan atmospheric probe (Huygens), is still the most sophisticated mission to be flown to the outer planets. It will allow achievement of the following measurement objectives that are necessary to attain the goals listed in COMPLEX's strategy for the exploration of the outer planets.[8] These measurement goals are to:

- Increase the spatial resolution of satellite coverage by repeated close flybys to allow geological assessment of the surface morphology of the icy satellites;
- Obtain long-term temporal sampling of the saturnian atmosphere to better understand its dynamical properties;
- Obtain higher-resolution sequential observations of ring structures to study wave phenomena and other ring aspects;
- Map the magnetosphere and understand its interactions with the rings, icy satellites, Titan, and the solar wind; and
- Measure the composition, structure, and circulation of Titan's atmosphere and characterize the physical properties and geographic variability of its surface.

The planned instruments and orbital sequences will allow synergistic observations of Saturn and will gather spatially and temporally dependent data adequate to fulfill many of the criteria delineated in the published Space Studies Board documents.[9]

With respect to deletion of cruise science in the restructured mission plan, COMPLEX notes that Jupiter and Saturn are similar objects. If observations of the jovian system were obtained with Cassini's instruments, then both the saturnian data set from Cassini and the jovian data set from Voyager/Galileo could be directly intercompared and, thus, the scientific return of each greatly enhanced. Therefore, COMPLEX recommends that the decision to delete all cruise science be revisited. An effort should be made to obtain a significant jovian data set, acquired one-half of a jovian year after the completion of the Galileo mission. COMPLEX recommends that NASA carefully assess the incremental cost of this part of the mission in relation to the scientific value of the jovian data and the increased engineering reliability that would be gained by early operation of the Cassini system.

Although the Cassini spacecraft has undergone considerable revision, it is COMPLEX's overall opinion that the restructured Cassini mission remains responsive to the scientific priorities set out in its report, *A Strategy for Exploration of the Outer Planets: 1986-1996*.[10] Significant though these changes are with respect to legitimate individual science objectives, the recommended modifications do not substantially compromise the primary mission objectives, which include the intensive study of the saturnian system as a whole.

COMPLEX reiterates its statement of March 30, 1992,[11] that, with current technology, any intensive study mission beyond the asteroid belt must be more than a Discovery-class mission. Although intermediate-size missions (larger than Discovery class, but smaller than Cassini) could undoubtedly achieve some of COMPLEX's objectives for comprehensive study of the saturnian system, they could not achieve many others.

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[8] Reference 6.

[9] Reference 6.

[10] Reference 6.

[11] Reference 5.

5
Congressional Testimony

5.1 Testimony on Priorities in Space Life Sciences Research

Space Studies Board member Robert H. Moser delivered the following testimony before the Task Force on Defense, Foreign Policy, and Space of the Committee on the Budget of the U.S. House of Representatives, on April 28, 1992.

Mr. Chairman, Ranking Minority Member, and members of the Task Force. Thank you for inviting me to testify at these important hearings on behalf of the Space Studies Board of the National Research Council (NRC). The Research Council is the operating arm of the National Academy of Sciences, chartered by Congress in 1863 to advise the federal government on matters of science and technology.

As an M.D., my experience with the NASA spaceflight program goes back to the days of Project Mercury. A biographical sketch of my professional background is attached. Over the years, I have served on a wide variety of panels and committees that advise NASA and the nation about research, health, and safety issues associated with the presence of humans in space. Currently, in addition to serving as a member of the NRC's Space Studies Board and that Board's Committee on Human Exploration, I am a liaison member of NASA's internal advisory Committee on Scientific Utilization of Space Station Freedom.

SPACE STUDIES BOARD OF THE NATIONAL RESEARCH COUNCIL

Established in 1958, the Space Studies Board is the National Research Council's primary advisory body concerning the U.S. civil space research program. It is the Board's responsibility to provide timely and objective advice both when requested to do so or when, in the view of the Board and the NRC, it is warranted and appropriate to do so. In representing the Space Studies Board, my testimony today will be limited to those issues associated with support of biomedical research in space and its role in the nation's space program. It is these issues that the Board has reviewed and assessed as recorded in its published reports, statements, and previous testimony to Congress.[1]

[1] For examples of previous Space Studies Board positions on biomedical and life sciences research, see: *A Strategy for Space Biology and Medical Science for the 1980s and 1990s* (NAP, 1987); *Assessment of Programs in Space Biology and Medicine—1991* (NAP, 1991); "Space Studies Board Position on Proposed Redesign of Space Station Freedom" (March 1991); "Space Studies Board Assessment of the Space Station Freedom Program" (March 1992); and testimony to the Senate Subcommittee on HUD Appropriations, Committee on Appropriations, by L. Dennis Smith, chair, Committee on Space Biology and Medicine, Space Science Board (May 1987).

BIOLOGICAL AND MEDICAL RESEARCH IN SPACE

At the outset, let me emphasize that from my perspective, the conduct of biological and biomedical research in space has one primary purpose—to support the national goal of a long-term human presence in space. In the absence of that goal, the space life sciences program would have an entirely different focus. There is an undeniable fascination in studying the effects of microgravity on plants, animals, and humans in space. Life as we know it has evolved in the presence of gravity. Thus, it comes as no surprise that all of Earth's living organisms show various abnormalities when exposed to the microgravity environment of space. Exploring the effects of microgravity on the development and maintenance of living systems is of considerable scientific interest. It is imperative, however, that national goals guide a research endeavor of this magnitude. Purely academic curiosity is an insufficient rationale for investing tax dollars on this scale.

As each of you knows, life sciences research conducted on the ground is expensive. There are a multitude of compelling reasons to make this investment—ranging from improving access to health care for all our citizens and thus improving the quality of life, to supporting the unending search for disease cures. Conducting life sciences research in space adds considerably to the cost and is not likely to help us achieve these notable goals. Thus it cannot be justified on the same grounds as ground-based research, nor should it be. At the risk of becoming repetitious, I would like to emphasize that the primary justification for space life sciences research is a commitment to long-term human exploration. Let me turn to a discussion of those issues on which the Space Studies Board has taken a series of public positions—the relationship to and importance of life sciences research in the U.S. civil space program.

NATIONAL GOALS AND THE CIVIL SPACE PROGRAM

In 1988, then-President Ronald Reagan put forth a Presidential Directive on National Space Policy. The policy, later reaffirmed by President Bush, states that "a fundamental objective guiding United States space activities has been and continues to be, space leadership."

Earlier this year, NASA Administrator Richard Truly issued *Vision 21—The NASA Strategic Plan* (NASA, January 1992). This multiyear plan is an implementation strategy for the goals contained in the Presidential Directive. Among those goals is an expansion of human activity beyond Earth orbit, including long-duration human exploration. The Board has made a number of statements concerning life sciences research in the context of that goal.[2]

In 1987, through its standing Committee on Space Biology and Medicine, the Board issued a comprehensive research strategy that focused on the program, experiments, and instruments that would be required to answer the many fundamental scientific questions that have been identified in this still emerging field of space science. The strategy report recognized the distinctiveness of this area of space research.

> Space medicine is unique in the context of the other space sciences—primarily because, in addition to questions of fundamental scientific interest, there is a need to address those issues that are more of a clinical or human health and safety nature. The authoring committee and the Space Science Board reached an important consensus in approving this report. That is, if this country is committed to a future of humans in space, particularly for long periods of time, it is essential that the vast number of uncertainties about the effects of microgravity on humans and other living organisms be recognized and vigorously addressed. Not to do so would be imprudent at best—quite possibly, irresponsible.

The committee advised that while some space life sciences research *is* clinical in nature, much of it is also of basic interest—for example, dealing with fundamental questions concerning the role of gravity in life processes. It pointed out that "in a properly framed strategy, basic and clinical research can complement one another." An important conclusion reached by the committee is the following:

> Space biology and medicine is in its infancy. Relatively few biological experiments have been flown, most of them have not been part of a larger research strategy, and few of them have been adequately controlled or replicated.

In 1992, even with the noteworthy achievements of successfully flying Spacelab-I and the International Microgravity Laboratory mission last year, the field is still in its infancy. Yes, there has been progress, but much remains to be done.

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[2] See reports, statements, and testimony cited in footnote 1.

PRIORITIES AND BENEFITS

Among other things, you asked me to comment on priorities in health and medical research funding and on the scientific return and cost-effectiveness of space-based life sciences research. The question is asked frequently: What can we learn from exposure to microgravity that will help us in diagnosing and treating disease on Earth? This is not an easy question—nor am I convinced it is appropriate. We know that plants, animals, and human beings are the creatures of gravity. We know that bone, muscle, the heart, lungs, and central nervous system are influenced significantly by gravity. I confess that I do not possess the imagination to envision what can be learned by prolonged exposure to microgravity that will help us solve our earth-bound medical problems. In this context, it would be most difficult for me to justify the enormous expense and risk of space-based life sciences research for that purpose. I am open to being convinced otherwise—but thus far, I have encountered no compelling evidence or arguments.

Discussion of priority setting among and between sciences has been enjoying particular prominence of late—deservedly so. In fact I would be remiss if I did not mention that the Board testified on this topic at another hearing to another committee this very morning.[3] In representing the Space Studies Board, it would not be appropriate for me to take a position on the priority to be accorded to space biology and medicine relative to the overall U.S. health and medical research enterprise. That is well beyond the purview of the Board and, as I have already stated, the goals are quite different. I can, however, discuss some relative priorities within the context of national goals and space biology and medicine in the context of human space exploration.

ASSESSMENT OF PROGRAMS IN SPACE BIOLOGY AND MEDICINE

In 1991, the Board issued a series of assessments of NASA's progress in implementing recommendations made concerning the various space research disciplines, including space biology and medicine. In discussing the major imperatives for research in space biology and medicine, the assessment categorized research topics relative to the urgency that would be dictated by proceeding with a space exploration initiative.

> The Space Exploration Initiative (SEI) envisions a sequential progression of human activities in space of many years duration. This places increased emphasis on implementation of the appropriate research strategies. Ironically, since a small number of Soviet astronauts have survived in low earth orbit for as long as a year, the perception has developed that there are no major physiological or psychological problems likely to preclude longer-term human exploration beyond low earth orbit. The fallacy of that assumption has been documented in previous reports and the current document reaffirms that conclusion.

Concerning physiological and psychological problems associated with an extended human presence in space, the committee presented them in a rough order of priority, relative to their importance to extended human space travel: bone, muscle, and mineral metabolism; cardiovascular and homeostatic functions; and sensorimotor integration. Psychosocial perturbations and exposure to radiation rank as equally important.

All physiological change in microgravity represents a homeostatic accommodation to this new environment. In reality, such an accommodation represents a maladaptation to Earth's gravity. Thus, although crews may do reasonably well during a long voyage, problems may arise when they reenter Earth's gravitational field.

The bone and muscle atrophy that occurs in the microgravity environment is a severe hurdle to overcome in achieving an extended human presence in space. While the cardiovascular system appears to function normally during short-term exposure to microgravity, clinically significant dysfunction is often apparent during readaptation to 1-G and is likely amplified with prolonged spaceflight. In addition, prolonged exposure to the altered loading conditions of microgravity is considered to be a potential cause of irreversible functional and structural changes. Results from experiments flown on SLS-I and reflown on SLS-II will help us to begin to understand these effects. Hormones that affect the cardiovascular system are of great importance and should be considered in the context of the cardiovascular changes that occur in space.

The changes in gravity-sensing nerve tissue that inevitably occur during a space mission lead to disturbances of sensorimotor function, including impaired spatial orientation as well as instability of gaze and motion systems. Provided a constant environment is maintained, the central nervous system adapts to these environmental changes within a few days. However, there are caveats to this assessment of relative risk. One is that gravito-inertial changes

[3] See statement of John A. Dutton, chair, Space Studies Board Task Group on Priorities in Space Research, to the Subcommittee on Science, Space, and Technology, U.S. House of Representatives, April 28, 1992.

occur at the most critical parts of a mission—during takeoff or landing. This would be an issue, for example, for crews landing on Mars, where a gravitational field about one third that of Earth will be encountered.

In addition to describing the physiological effects of microgravity on humans in space, a host of reports have discussed the recognition of psychosocial problems during long-duration missions such as those planned for Space Station Freedom, a manned lunar base, a voyage to Mars, or a martian outpost.[4] Current research using analogue environments and other means do not provide convincing evidence that missions longer than one year will be tolerated in the closed-limited environments that are contemplated for prolonged space missions. Psychosocial issues may be critical limiting factors in the exploration of space. This is another area that calls for much research.

Another category that requires investigation before humans embark on any long-duration space voyage is the effects of the radiation environment beyond the magnetosphere. The radiation environment of space is considerably less benign than that on Earth. Planning for extended human sojourns in space mandates that we have quantitative knowledge about the dose rates, and the types of radiation that will be encountered, and the shielding that will be required. Here again, research on Earth and in space will be required.

The areas I have just discussed are those that the Board and its Committee on Space Biology and Medicine have determined to be most urgent and critical to supporting a Space Exploration Initiative.

To summarize what I have said in this part of the testimony—there are several critically important areas in space biomedical and behavioral research that must be adequately supported so that the United States can safely and successfully realize a goal of long-term human space exploration. I want to emphasize that a long period of time and effort will be required for the satisfactory pursuit and resolution of all these problems. As the Board indicated in both its March 1991 and March 1992 statements concerning Space Station Freedom, we concur with the recommendation of the Augustine Committee that the primary objective of a space station should be life sciences research.[5] That is, "A space-based laboratory is required to study the physiological consequences of long-term spaceflight."[6]

COOPERATIVE RESEARCH

One way to maximize the return on investment in research is through various modes of cooperative research, with foreign partners, private concerns, and between federal agencies. To the extent that there are shared or compatible goals, the Board and its Committee on Space Biology and Medicine strongly endorse this approach. For example, the National Institutes of Health is this nation's primary supporter of the biomedical research enterprise. Over the past several years, there have been increasing efforts between NIH and NASA to identify and collaborate in support of areas of mutual benefit to the two agencies. Dr. Bernadine Healy, director of the NIH, testified before the House Committee on Science, Space, and Technology last October:[7]

> NASA's life sciences program and the biomedical research activities at the NIH are complementary in that both are concerned with human health. NIH's research focuses on the full range of sciences relevant to improving the health of Americans on Earth. NASA's life sciences efforts are centered primarily on the health of astronauts today and tomorrow. . . . While examining the direct effects of space flight on the human body is the primary concern of NASA's medical researchers, NIH conducts and supports a wide range of studies relating to these many important phenomena. In particular, both cardiovascular research and studies on bone demineralization or osteoporosis are major and important focuses for NIH investigators because of their devastating effects on the health of Americans here on Earth. . . . Bone demineralization is of great interest to NIH researchers. It is pervasive among elderly women. The problems of osteoporosis and bone demineralization affect an estimated 24 million individuals here on Earth and indeed it is a debilitating condition which is pervasive among long-term space travelers as we have determined.

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[4] *Life Beyond the Earth's Environment—The Biology of Living Organisms in Space* (NAS, 1979); *A Strategy for Space Biology and Medical Science for the 1980s and 1990s* (NAP, 1987); *Leadership and America's Future in Space* (NASA, A Report to the Administrator by Sally K. Ride, August 1987); *Exploring the Living Universe—A Strategy for Life Sciences* (NASA, Washington, D.C., June 1988); and *Space Science in the Twenty-First Century—Imperatives for the Decade 1995-2015—Overview* and *Life Sciences* volumes (NAP, 1988).

[5] *Report of the Advisory Committee on the Future of the U.S. Space Program*, Superintendent of Documents (GPO), December 1990.

[6] Committee on Space Biology and Medicine Strategy previously cited and Space Studies Board letter to Joseph Alexander, assistant associate administrator, Office of Space Science and Applications, NASA Headquarters, December 12, 1990; Space Station Summer Study Report, SESAC Task Force on Scientific Uses of a Space Station, NASA, March 1986.

[7] Hearing before the Subcommittee on Space of the Committee on Science, Space, and Technology, U.S. House of Representatives, October 23, 1991.

Another example for potential collaboration between federal agencies are facilities supported by the Department of Energy such as the BEVALAC, which has the capability of providing for study of very high-Z particles and their biological effects.

With respect to our foreign partners, including the former Soviet Union, Europe, Japan, Canada, and others, we believe it would be mutually beneficial and of utmost importance to seek ways to enhance cooperative efforts and exploit all available spaceflight opportunities. Resources are limited for all of us.

CONCLUSION

In the early days, most physicians involved in monitoring orbiting Mercury and Gemini astronauts were nervous. Human experience in the microgravity environment, even for short periods, was *terra incognita*. There was no precedent, no background of information about human physiological and psychological performance in the weightless state.

Thus we were obliged to monitor physiological function. We asked the brilliant engineers at NASA to give us two leads of EKG, and the capability to monitor blood pressure, heart rate, respiration rate, and body temperature. Psychological performance was observed by listening to communications—rarely by speaking directly with astronauts. As a result, there was a forced-draft effort to create equipment that would enable us to observe—in real time—psychological performance. From this endeavor, we learned about telemetering ECGs. We developed miniaturized diagnostic equipment. New, strong, lightweight materials were created, and many other developments occurred. All these discoveries had significant spin-offs related to Earth-bound medicine. But it is critical to realize that these remarkable devices and materials were created to monitor astronauts—not to improve the well-being of Earth-bound patients. It is essential to appreciate this distinction. The primary purpose of physiological, psychological, and radiation-effect research in space is to learn enough to provide some reasonable assurance that crews can survive and function in this most unforgiving of environments. Prolonged space faring, as would be involved in any human mission to Mars, remains *terra incognita*.

Of course we have learned things from U.S. and [then] Soviet missions. This information has raised many alarms—emphasizing that as responsible medical scientists we must conduct much more Earth-based and space-based research before we can commit crews to prolonged spaceflight, and still be able to sleep at night ourselves.

Undoubtedly, there will be benefits derived from space life sciences research that will be beneficial to patients on Earth. But again, this will be information largely peripheral to the sole purpose of space medicine—to learn enough to ensure reasonable lack of risk to space-faring crews. Benefits derived for Earth-bound medicine must not be construed as the primary driver of space medicine.

Finally, if indeed the people of this nation decide not to send crews to explore the universe, I personally will no longer remain involved in this adventure. Without prolonged human spaceflight remaining as a high priority on the American agenda, there is no compelling justification for space medicine.

5.2 Testimony on Setting Priorities in Space Research

The following testimony was delivered by Space Studies Board member John A. Dutton before the Subcommittee on Science of the Committee on Science, Space, and Technology of the U.S. House of Representatives, on April 28, 1992. Dr. Dutton is also chair of the Board's Task Group on Priorities in Space Research.

Mr. Chairman, members of the Subcommittee. Thank you for inviting me to testify at these important hearings on behalf of the Task Group on Priorities in Space Research, a committee of the Space Studies Board, National Research Council.

As you know, we have just released a report, *Setting Priorities for Space Research—Opportunities and Imperatives* (National Academy Press, Washington, D.C., 1992). That report is the culmination of a two-year study which focused on whether the space research community should have a role in setting priorities for those scientific objectives and initiatives which comprise the space science and applications component of the nation's civil space program. Our conclusion was a resounding "Yes." Not only is it desirable—it is imperative. That it took nearly two years to convince ourselves, the Board, and other colleagues from the space community of the validity of this conclusion indicates the sensitivity and difficulty of this issue.

In our deliberations, we were inspired by a quotation from Metternich brought to us by a task group member, Buddy McKay—one of your former colleagues, now Lt. Governor of Florida.

> [Policy] is like a play in many acts, which unfolds inevitably once the curtain is raised. To declare that the performance will not take place is an absurdity. The play will go on, either by means of the actors . . . or by means of the spectators who mount the stage.

In my remarks today, I will set the context for our report, give a brief overview of its conclusions, and outline how we plan to approach the second phase of this study—by far the more difficult enterprise.

THE KEY QUESTIONS IN SETTING AN AGENDA

Each of you is well aware that, in sum, the requirements and opportunities competing for federal support far exceed available funding. We know that too. We also know that scientific research is an investment in this nation's future, not an entitlement program.

In our report, we document a wide array of remarkable achievements of the U.S. space research program over the past thirty years. We go on to describe some of the abundant opportunities that exist now and for the future. NASA charts depicting funding levels required just to complete the ongoing program, let alone begin new projects, are a graphic reminder of the very real need to make difficult choices. The community of scientists engaged in research in space must reach a consensus on priorities and contribute to the formulation of an agenda for space research and for the space program. Such an agenda and the priorities it represents must respond to national needs and to the larger priorities imposed by national goals.

The two key questions in space research, as in most continuing endeavors, are: What should we do? How should we do it? We set our agenda with the answers to these questions—the priorities that we choose reflect our goals and our values. Careful consideration and formulation of assumptions and priorities for the scientific research program and the overall space program that supports it will enable us to better serve national goals, compel effective action, achieve the maximum return on our national investment, and foster public pride and confidence.

THE HIERARCHY OF PRIORITIES

Let me state my personal view of how the issues addressed by our report fit within the context of the national decision-making process that creates the agenda for scientific activities. These ideas will be discussed as we proceed with the second phase of our study. Priorities for space research or for a national science program appear within a hierarchy that ranges from national goals to specific research projects.

- *National Goals*—At the top of the hierarchy are national goals and objectives, such as developing deeper understanding of the world around us, strengthening education of young citizens, enhancing economic vitality, and preserving the environment. Priorities for such goals obviously evolve, but the time scale on which they are pursued will usually be decades or longer and may extend to centuries.

- *Strategic Endeavors*—Next are the strategic endeavors or initiatives that encompass or facilitate a collection of activities intended to contribute to the achievement of national goals. Examples might include the fight against disease, the study of global change induced by human activities, the development of enhanced computer and information technology, the scientific exploration of the solar system, or the conservation of energy. Strategic endeavors are pursued over time scales of years or decades.
- *Specific Initiatives and Activities*—At the third level are the initiatives and continuing activities through which we actually achieve the aims of strategic endeavors. These include specific research programs, space research missions, technology development programs, or development of new research facilities. The conceptualization, development, and implementation of these initiatives may take years, or, perhaps, more than a decade.

In order to consider priorities effectively, we must divide these specific initiatives into two categories: conceptual or potential efforts and programmatic activities. We formulate the agenda for future programmatic activities by selecting those potential efforts to pursue—we thus decide what we shall do. In setting a programmatic agenda, we determine how we shall do it.

In space research, programmatic activities include ongoing research and the design, construction, and flight of spacecraft and the use of data from such flights. Examples of programmatic activities include implementing mature mission proposals such as those for the Advanced X-ray Astronomy Facility (AXAF) or the Earth Observing System (EOS). Conceptual efforts concentrate on developing new ideas and new approaches for attacking scientific questions; they examine the possibilities for utilizing technological advances to obtain scientific information from space. In brief, they explore mission concepts, refining them until they evolve into proposals for programmatic activities. Developmental or conceptual efforts might be typified by studies of an astronomical facility on the moon, a suite of robotic missions to install scientific instruments on Mars, a mission to Pluto, or a constellation of geosynchronous satellites for continuing surveillance of the Earth and its atmosphere.

Within space research, priorities for programmatic activities have been developed in recent years by the Space Science and Applications Committee using a methodology created by its predecessor, the Space and Earth Science Advisory Committee.[1] So far, there has been no formal effort to set priorities among developmental efforts across all of space research. The disciplinary committees of the Space Studies Board have regularly set forth long-range research strategies with scientific goals and objectives for each of the subdisciplines of space research. These have not, however, been refined into an overall development plan with clear priorities. It is the difficult task of recommending priorities for such a long-range development program that we address in our report, *Setting Priorities for Space Research.* We need to develop a procedure for creating our agenda a decade or so in advance so that we know with confidence precisely what we intend to do, so that we can concentrate on the highest-priority endeavors.

I would argue that the extent to which the scientific community and public officials can shape an effective national program in space research depends in part on how clearly we understand and can enunciate the higher-level goals or objectives which we hope to serve. If we are vague about our national goals and strategic priorities, then it is difficult to focus development and programmatic activities to achieve them. If our national goals and strategic priorities shift about from one emphasis to another, then we shall waste money and effort in program development and execution as we start projects and then later cancel them. In our report we discuss the importance of fundamental assumptions in shaping priorities—these assumptions elucidate the basic motivations for what we are trying to accomplish and they must derive from, and serve the higher purposes of, space research or science. The more clearly those purposes are formulated, the more effective our system of priorities for scientific endeavors will be.

The remainder of my remarks are based on discussions and conclusions of the Priorities Task Group.

INFORMATION, KNOWLEDGE, AND UNDERSTANDING

We examined the role of fundamental assumptions in shaping the civil space program. For some time, the objectives of the space research community and those of the broader space program have been in conflict. Apollo

[1] For a description of this methodology see: *The Crisis in Space and Earth Sciences—A Time for a New Commitment* (NASA Advisory Council, 1986); also John A. Dutton and Lawson Crowe, "Setting Priorities Among Scientific Initiatives," *American Scientist* **76**:599-603 (1988).

demonstrated national technological superiority at a critical time. A fundamental assumption of the civil space program developed in that era asserts that it is human destiny to explore the Solar System and perhaps beyond. New realities of international competition, domestic politics, and economics suggest the need to examine our assumptions to ensure that space research and the space program contribute effectively to national vitality.

We believe that the imperative driving scientific research is the acquisition of knowledge and understanding. Thus the collection of data, the creation of information through its analysis, and the subsequent development of insight and understanding should be the key governing objectives for scientific research in space and for the broader space program. We believe that the nation would benefit if space research and much of the space program emphasized the acquisition of information and knowledge and the development of insight and understanding. Adopting the acquisition of information that cannot be obtained on Earth as the primary purpose of space activities is compatible with national needs to develop advanced technologies and capabilities. Most significantly, such a purpose provides clear objectives for future development of the human spaceflight program.

ECONOMIC REALITIES AND THE MANAGEMENT OF AVAILABLE RESOURCES

Today, as federal dollars become increasingly scarce, demands for clear benefits from public investments and for effective use of available resources confront the space science and applications community.

Two trends in public policy offer both challenge and opportunity to space science. First, there appears to be an increased willingness to support activities primarily producing broad social benefits, as evidenced by policy and action motivated by concerns for clean water and air, for protecting the environment, and for maintaining wilderness, wildlife, and habitats. Second, there is an increasing demand for publicly supported activities to provide explicit evidence that the benefits to be achieved merit the costs. Responding to these demands requires careful thought to demonstrate how space research or other scientific effort that fundamentally serves to augment knowledge and understanding contributes to society; it requires careful analysis to answer questions such as: In what way and by how much does space research further national objectives?

Economic benefits have been cited as a rationale for space research since the inception of the U.S. civil space program, yet the precise meaning of "economic benefit" has not always been clear. The narrowest definition would include strictly commercial activity that is profitable in the business sense. The case most often cited is that of commercial communications satellites, in which economic benefits can be defined as the value consumers place on the service and are measured by industry revenues.

We do not offer a formal cost-benefit analysis for scientific research in space. That was both beyond our charge and is difficult to do. However, from the perspective of setting priorities for space research initiatives, many requirements of cost-benefit analysis are instructive. Both those who propose research initiatives and those who review them should, as far as possible, identify all costs and benefits, determine the necessary conditions for success, estimate the probabilities and the consequences of failure, and specify the expected outcomes. While we are aware that many people object to any attempt to quantify science and knowledge, we believe this sort of analysis must be factored into any effective priority-setting procedure.

In parallel with demonstrating the benefits of space research, we must be sure that we use the available resources wisely and efficiently. Many observers have emphasized that space research efforts seem to cost too much, take too long, and all too often fail to meet their original objectives. In recent years, we have forced scientific missions into launch modes that dramatically increased their costs and reduced their effectiveness. We diffuse our support for science by attributing scientific motivations to efforts that, while they serve legitimate public purposes, are essentially nonscientific. In our report, we discuss some of the lessons we have learned in three decades of space research and some of their implications for the future.

RATIONALE FOR SETTING PRIORITIES

We argue that there are two principal justifications for working toward a consensus and recommending priorities: First, consensus is politically compelling. If scientists can demonstrate that their agenda responds to national needs and to scientific imperatives, then they can argue effectively for an adequate share of resources and for an orderly progression through the suite of initiatives endorsed by the community. Second—as Metternich said—if the players will not act, then the spectators will take the stage. If scientists engaged in space research cannot, or will not, set priorities among opportunities, then others whose own goals may be quite different will take the stage and make the decisions. Passivity or disarray on the part of the scientists presents the political process with the opportunity, indeed the necessity, to make choices, some of which may not be in the best interests of science.

In order to prepare an effective developmental agenda, we will need a sophisticated system of priorities. A simple ranked list will not be sufficient. We envision a hierarchical scheme, with certain categories of activities given a higher priority than others. The categories in such a scheme might include support for basic research and scientific infrastructure, followed by mandatory efforts, large initiatives, and incremental efforts that are part of the forward march of science. The relative priorities in such a scheme can be presented as a matrix, with the columns representing categories and containing activities listed by relative priority within the category.

There are not now, nor are there ever likely to be, sufficient resources to do everything we would like to do. It is time for the proponents and the recipients of federal research support to step up to the challenge of participating in the decision-making debate. As scientists and engineers, we have the unique capability of examining our own scientific and technological goals and objectives from a vantage point as experts in the field. We must, as encouraged by Congressman Brown in a recent address at the National Academy of Sciences, provide policy makers with our best assessment of priority ordering based on "unadulterated peer-reviewed judgment of scientific merit."

COUNTER-ARGUMENTS TO THE COUNTER-ARGUMENTS

In the course of our study and since the publication of our report, we have encountered a remarkably uniform set of arguments against scientists participating in setting priorities. Not surprisingly, some find the notion of setting priorities threatening. Anticipating counter-arguments, we offered a response to those arguments in our report. Below, I list some of the objections, and then our counter-arguments to them.

- *There will be losers.* Yes, there will be, just as there are losers now. Consensus in the scientific community along with effective advocacy will, in all likelihood, produce more funds and stable funding patterns and hence strengthen science and increase opportunities for the recommended initiatives. Without a process that identifies and promotes good science and strong initiatives, resources are scattered and the strong subsidize the weak.
- *Recommending priorities is too difficult, too contentious.* Yes, it is difficult. But we believe it can be accomplished through a formal process in which competing initiatives are judged uniformly according to explicit criteria, preferably on the basis of written material that specifically addresses the stated criteria. Again, if scientists find it too difficult to create an agenda for space research, then, as argued above, others will do it for them.
- *The community will not be able to maintain consensus because those who lose will subvert the process by lobbying policy makers and Congress directly.* We argue that rather than seeking to restore initiatives that have been abandoned, those who lose out in the process would be better advised to develop more competitive initiatives.
- *Setting priorities will be counterproductive because the community will tear itself apart.* We believe that insisting on a fair, open, and formal process will, in the end, serve both individual scientists and science at large. If the space research community is to be taken seriously by others, then it should accept responsibility for its own future.
- *The low-priority initiatives will not be done.* Exactly—that is the purpose of setting priorities. When resources are limited, they should be directed toward the highest-priority endeavors.
- *Scientists cannot make political judgments.* We believe that in arguing for initiatives, scientists should be sensitive to national goals and political realities, just as we expect that politicians in considering scientific initiatives should be sensitive to scientific merit. Since scientists expect support from taxpayers, they should be willing to explain to the public why some initiatives better serve national purposes.

THE DIFFICULT PART

Having begun the second phase of our study, we are well aware that the most difficult aspect of our endeavor lies ahead. Over the next year, we will be developing a procedure for recommending priorities that will contribute to the creation of a vigorous long-range space research agenda. We understand that for such a procedure to be successful, it must be accepted by the space research community at large while at the same time serving as a meaningful source of practical, reasoned advice to decision makers. It is our intention to actively involve the space research community in the development and testing of the methodology and implementation plan we create. That dialogue began earlier this year at a symposium marking the release of our phase-one report.

Many issues and questions must be addressed and answered. For example:

- What are the appropriate criteria for determining priorities in developing a long-range agenda for space research or for other scientific endeavors?
- Who should be responsible for administering the process that is finally recommended?
- What will be the time schedule for the evaluation process and subsequent priority recommendations?
- To whom should evaluators' recommendations be directed: Congress, NASA, the Space Council, or . . . ?
- How will the process provide for making choices within disciplines as well as across space research disciplines?
- Is it realistic to suggest that science can be subjected to any sort of cost-benefit analysis?
- How can we determine what budget limits (minimum and maximum), if any, should be placed on the totality of efforts considered in a developmental agenda?
- To what extent should we narrow the choices as we approach setting the programmatic agenda?

These are just a few of the questions we must answer. There will be more questions and more criticisms. Clearly, we have set ourselves a difficult task. However, we believe it would be a serious mistake not to try. Helping to fashion the appropriate criteria for making these difficult choices is, we believe, a responsibility of the space research community. The community is capable of making the sophisticated judgments necessary to foster a vital and robust space research program. We believe it must do so.

6
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